

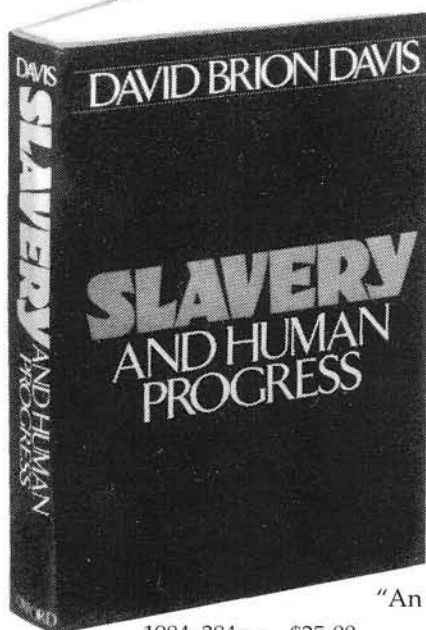
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The *American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 (202-544-2422) and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405 (812-335-7609).

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes of \$40,000 and above, \$60.00 annually; \$30,000–\$39,999, \$55.00; \$20,000–\$29,999, \$47.00; \$15,000–\$19,999, \$40.00; \$10,000–\$14,999, \$30.00; below \$10,000, students, and joint memberships \$20.00; associate (nonhistorian) \$30.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 90: Class I, *American Historical Review* only, United States \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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Contents

VOLUME 90 • NUMBER 2 • APRIL 1985

Articles

- Order and Chaos in Early America: Political and Social Stability
in Pre-Restoration Virginia, BY JON KUKLA 275
- The Hypothesis of Middle-Class Formation in Nineteenth-Century America:
A Critique and Some Proposals, BY STUART M. BLUMIN 299
- Capitalism and the Origins of the Humanitarian Sensibility, Part 1,
BY THOMAS L. HASKELL 339
- Why Foreign Oil Companies Shifted Their Production from Mexico
to Venezuela during the 1920s, BY JONATHAN C. BROWN 362

Reviews of Books

GENERAL

- BARBARA W. TUCHMAN. *The March of Folly: From Troy to Vietnam*. By Charles Tilly 386
- MICHEL FOUCAULT. *Histoire de la sexualité*. Volume 2, *L'Usage des plaisirs*; volume 3, *Le souci de soi*.
By Vern L. Bullough 387
- FRANCIS OAKLEY. *Omnipotence, Covenant, and Order: An Excursion in the History of Ideas from Abelard to Leibniz*.
By Eugene Webb 388
- ROBERT EDEN. *Political Leadership and Nihilism: A Study of Weber and Nietzsche*. By Willi Paul Adams 388
- STEVEN B. SMITH. *Reading Althusser: An Essay on Structural Marxism*. By Barry M. Katz 389
- NOËL O'SULLIVAN. *Fascism*. By Gilbert Allardyce 390
- RICHARD A. VAN ORMAN. *The Explorers: Nineteenth-Century Expeditions in Africa and the American West*.
By John L. Allen 390
- WILLIAM H. BLANCHARD. *Revolutionary Morality: A Psychosexual Analysis of Twelve Revolutionists*.
By Elizabeth Wirth Marvick 391
- RICHARD D. MANDELL. *Sport: A Cultural History*.
By Allen Guttman 392

ANCIENT

- W. ROBERT CONNOR. *Thucydides*.
By Charles W. Fornara 393
- JANE HORNBLOWER. *Hieronymus of Cardia*; KOSTAS BURASELIS. *Das hellenistische Makedonien und die Ägäis: Forschungen zur Politik des Kassandros und der drei ersten Antigoniden (Antigonos Monophthalmos, Demetrios Poliorketes und Antigonos Gonatas) im Ägäischen Meer und in Westkleinasien*.
By Roger B. McShane 393
- PATRICIA COX. *Biography in Late Antiquity: A Quest for the Holy Man*. By Michael M. Sage 394
- RICHARD J. A. TALBERT. *The Senate of Imperial Rome*.
By G. Michael Woloch 395
- RALPH MERRIFIELD. *London: City of the Romans*.
By Edward N. Luttwak 396

MEDIEVAL

- PETER CLASSEN. *Studium und Gesellschaft im Mittelalter*.
By John W. Baldwin 397
- TIMOTHY GREGORY VERDON, editor. *Monasticism and the Arts*.
By William R. Cook 397

RICHARD KRAUTHEIMER. <i>Three Christian Capitals: Topography and Politics</i> . By Gregory T. Armstrong	399	DOROTHY THOMPSON. <i>The Chartists: Popular Politics in the Industrial Revolution</i> . By Duane C. Anderson	414
H. R. LOYN. <i>The Governance of Anglo-Saxon England, 500–1087</i> . By William A. Chaney	399	ANTHONY S. WOHL. <i>Endangered Lives: Public Health in Victorian Britain</i> . By John Norris	415
PIERRE RICHE. <i>Les Carolingiens: Une famille qui fit l'Europe</i> . By Archibald R. Lewis	400	MARK GIROUARD. <i>Victorian Pubs</i> . By Peter Bailey	416
JOHN C. SHIDELER. <i>A Medieval Catalan Noble Family: The Montcadas, 1000–1230</i> . By Paul Freedman	401	STEVEN TISCHLER. <i>Footballers and Businessmen: The Origins of Professional Soccer in England</i> . By Tony Mason	416
H. E. J. COWDREY. <i>The Age of Abbot Desiderius: Montecassino, the Papacy, and the Normans in the Eleventh and Early Twelfth Centuries</i> . By James M. Powell	401	A. W. BRIAN SIMPSON. <i>Cannibalism and the Common Law: The Story of the Tragic Last Voyage of the Mignonette and the Strange Legal Proceedings to Which It Gave Rise</i> . By Eugene L. Rasor	417
KLAUS HERBERS. <i>Der Jakobuskult des 12. Jahrhunderts und der "Liber Sancti Jacobi": Studien über das Verhältnis zwischen Religion und Gesellschaft im hohen Mittelalter</i> . By John B. Freed	402	JAMES GOLDRICK. <i>The King's Ships Were at Sea: The War in the North Sea, August 1914–February 1915</i> . By Paul Guinn	417
GÉRARD SIVÉRY. <i>L'Économie du royaume de France au siècle de Saint Louis, vers 1180–vers 1315</i> . By Theodore Evergates	403	STEPHEN KOSS. <i>The Rise and Fall of the Political Press in Britain. Volume 2, The Twentieth Century</i> . By Alfred F. Havighurst	418
MICHAEL BURLEIGH. <i>Prussian Society and the German Order: An Aristocratic Corporation in Crisis, c. 1410–1466</i> . By Steven Rowan	403	HENRY PELLING. <i>The Labour Governments, 1945–51</i> . By Trevor Burridge	418
JENS E. OLESEN. <i>Unionskrige og staendersamfund: Bidrag til nordens Historie i Kristian I's regeringstid, 1450–1481</i> [The War of Union and the Estates Society: A Contribution to Scandinavian History in the Reign of Kristian I, 1450–81]. By G. Bernhard Fedde	404	MICHAEL LYNCH. <i>Edinburgh and the Reformation</i> . By Arthur H. Williamson	419
MODERN EUROPE		THOMAS J. MORRISSEY. <i>Towards a National University: William Delany, S.J., 1835–1924; An Era of Initiative in Irish Education</i> . By Hugh F. Kearney	420
BERNADOTTE E. SCHMITT and HAROLD C. VEDELER. <i>The World in the Crucible, 1914–1919</i> . By Modris Eksteins	405	CHARLES TOWNSHEND. <i>Political Violence in Ireland: Government and Resistance since 1848</i> . By Blanche M. Touhill	421
CAROLE FINK. <i>The Genoa Conference: European Diplomacy, 1921–1922</i> . By William E. Scott	406	ROBERT DESCIMON. <i>Qui étaient les Seize? Mythes et réalités de la Ligue parisienne, 1585–1594</i> . By Eli Barnavi	421
MONTY NOAM PENKOWER. <i>The Jews Were Expendable: Free World Diplomacy and the Holocaust</i> . By Carole Fink	406	GEORGE HUPPERT. <i>Public Schools in Renaissance France</i> . By Richard L. Kagan	422
DAVID HAPGOOD and DAVID RICHARDSON. <i>Monte Cassino</i> . By Robert W. Coakley	407	JEAN-MARIE CONSTANT. <i>Les Guise</i> . By James Eastgate Brink	423
ALAN S. MILWARD. <i>The Reconstruction of Western Europe, 1945–51</i> . By Sally Marks	408	DAVID BUISSET. <i>Henry IV</i> . By Sharon Kettering	424
HORST LADEMACHER, editor. <i>Gewerkschaften im Ost-West Konflikt: Die Politik der American Federation of Labor im Europa der Nachkriegszeit</i> . By Robert W. Sellen	408	OREST RANUM. <i>Artisans of Glory: Writers and Historical Thought in Seventeenth-Century France</i> . By Joseph Klaitis	424
J. F. C. HARRISON. <i>The English Common People: A Social History from the Norman Conquest to the Present</i> . By Richard S. Tompson	409	HANS-JÜRGEN LÜSEBRINK. <i>Kriminalität und Literatur im Frankreich des 18. Jahrhunderts: Literarische Formen, soziale Funktionen und Wissenskonsituenten von Kriminalitätsdarstellung im Zeitalter der Aufklärung</i> . By Michael R. Weissner	425
J. A. SHARPE. <i>Crime in Seventeenth-Century England: A County Study</i> . By J. S. Cockburn	410	MAURICE HUTT. <i>Chouannerie and Counter-Revolution: Puisaye, the Princes, and the British Government in the 1790s</i> . By Harvey Mitchell	426
J. T. CLIFFE. <i>The Puritan Gentry: The Great Puritan Families of Early Stuart England</i> . By Lawrence Kaplan	410	CISSIE FAIRCHILDS. <i>Domestic Enemies: Servants and Their Masters in Old Regime France</i> ; SARAH C. MAZA. <i>Servants and Masters in Eighteenth-Century France: The Uses of Loyalty</i> . By Thomas M. Adams	427
ROBIN CLIFTON. <i>The Last Popular Rebellion: The Western Rising of 1685</i> . By Gerald M. Straka	411	SUSAN BACHRACH. <i>Dames Employées: The Feminization of Postal Work in Nineteenth-Century France</i> . By Marilyn J. Boxer	428
J. A. W. GUNN. <i>Beyond Liberty and Property: The Process of Self-Recognition in Eighteenth-Century Political Thought</i> . By Robert A. Smith	412	THOMAS R. OSBORNE. <i>A Grande Ecole for the Grands Corps: The Recruitment and Training of the French Administrative Elite in the Nineteenth Century</i> . By Thomas D. Beck	429
BREAN S. HAMMOND. <i>Pope and Bolingbroke: A Study of Friendship and Influence</i> . By R. William Weisberger	413	CHARLES H. POUTHAS et al. <i>Démocratie, réaction, capitalisme, 1848–1860</i> . By Julian Archer	430
DOREEN M. ROSMAN. <i>Evangelicals and Culture</i> . By James Nicoll Cooper	413	JOSEPH JONES. <i>The Politics of Transport in Twentieth-Century France</i> . By James M. Laux	431
JOHN M. MACKENZIE. <i>Propaganda and Empire: The Manipulation of British Public Opinion, 1810–1960</i> . By Patrick Brantlinger	414	LINDA MARTZ. <i>Poverty and Welfare in Habsburg Spain</i> . By Ellen G. Friedman	431

- SHLOMO BEN-AMI. *Fascism from Above: The Dictatorship of Primo de Rivera in Spain, 1923-1930*.
By Douglas W. Foard 432
- R. HAVENAAR. *De NSB tussen nationalisme en "volkse" solidariteit: De vooroorlogse ideologie van de Nationaal-Socialistische Beweging in Nederland* [The NSB between Nationalism and "Folkish" Solidarity: The Prewar Ideology of the National Socialist Movement in the Netherlands].
By Werner Warmbrunn 432
- MERJA-LIISA HINKKANEN-LIEVONEN. *British Trade and Enterprise in the Baltic States, 1919-1925*.
By Derek H. Aldcroft 433
- BENGT NILSON. *Handelspolitik under skärpt konkurrens: England och Sverige, 1929-39* [Trade Policy under Tightened Competition: Britain and Sweden, 1929-39].
By Raymond E. Lindgren 434
- PER G. ANDREEN. *Finland i brännpunkten, mars 1940-juni 1941* [Finland in the Focal Point, March 1940-June 1941].
By Roberta G. Selleck 434
- HERBERT DAVID RIX. *Martin Luther: The Man and the Image*.
By Nelson H. Minnich 435
- R. PO-CHIA HSIA. *Society and Religion in Münster, 1535-1618*.
By Miriam Usher Chrisman 436
- PAUL WARMBRUNN. *Zwei Konfessionen in einer Stadt: Das Zusammenleben von Katholiken und Protestanten in den paritätischen Reichsstädten Augsburg, Biberach, Ravensburg und Dinkelsbühl von 1548 bis 1648*. By Louis J. Reith 437
- THOMAS MAX SAFLEY. *Let No Man Put Asunder: The Control of Marriage in the German Southwest; A Comparative Study, 1550-1600*. By Lawrence G. Duggan 438
- DAVID WALSH. *The Mysticism of Innerworldly Fulfillment: A Study of Jacob Boehme*. By Carter Lindberg 439
- PETER SCHMIDT. *Das Collegium Germanicum in Rom und die Germaniker: Zur Funktion eines römischen Ausländerseminars, 1552-1914*. By R. Po-Chia Hsia 440
- FRANKLIN KOPITZSCH. *Grundzüge einer Sozialgeschichte der Aufklärung in Hamburg und Altona*.
By T. C. W. Blanning 440
- GUNDA MAIRBÄURL. *Die Familie als Werkstatt der Erziehung: Rollenbilder des Kindertheaters und soziale Realität im späten 18. Jahrhundert*. By Jean H. Quataert 441
- KURT G. A. JESERICH et al., editors. *Deutsche Verwaltungsgeschichte. Volume 1, Vom Spätmittelalter bis zum Ende des Reiches; volume 2, Vom Reichsdeputationshauptschluss bis zur Auflösung des Deutschen Bundes*.
By James Allen Vann 442
- HERBERT OBENAU. *Anfänge des Parlamentarismus in Preussen bis 1848*. By Donald J. Mattheisen 444
- ROGER FLETCHER. *Revisionism and Empire: Socialist Imperialism in Germany, 1897-1914*. By Geoff Eley 444
- HERMANN SCHÄFER. *Regionale Wirtschaftspolitik in der Kriegswirtschaft: Staat, Industrie und Verbände während des Ersten Weltkrieges in Baden*. By Michael Geyer 445
- WALTER SCHWENGLER. *Völkerrecht, Versailler Vertrag und Auslieferungsfrage: Die Strafverfolgung wegen Kriegsverbrechen als Problem des Friedensschlusses, 1919/20*.
By Edward W. Bennett 446
- HERMANN WEBER. *Kommunismus in Deutschland, 1918-1945*.
By Franklin C. West 446
- KLAUS J. BADE, editor. *Auswanderer, Wanderarbeiter, Gastarbeiter: Bevölkerung, Arbeitsmarkt und Wanderung in Deutschland seit der Mitte des 19. Jahrhunderts*. By Walter Struve 447
- PETER STADLER. *Der Kulturkampf in der Schweiz: Eidgenossenschaft und Katholische Kirche im europäischen Umkreis, 1848-1888*. By Allan Mitchell 448
- ÉVA SOMOGYI. *Vom Zentralismus zum Dualismus: Der Weg der deutschösterreichischen Liberalen zum Ausgleich von 1867*.
By S. B. Vardy 449
- WALTER B. SIMON. *Österreich, 1918-1938: Ideologien und Politik*. By Martin Kitchen 450
- RADOMIR V. LUZA. *The Resistance in Austria, 1938-1945*.
By Klemens Von Klemperer 450
- M. E. MALLETT and J. R. HALE. *The Military Organization of a Renaissance State: Venice c. 1400 to 1617*.
By William S. Maltby 451
- J. N. STEPHENS. *The Fall of the Florentine Republic, 1512-1530*.
By Lauro Martines 452
- EDWARD L. GOLDBERG. *Patterns in Late Medici Art Patronage*.
By Jerry H. Bentley 453
- VINCENZO FERRONE. *Scienza natura religione: Mondo newtoniano e cultura italiana nel primo settecento*.
By William R. Shea 453
- ANGELO TAMBORRA. *Garibaldi e l'Europa: Impegno militare e prospettive politiche*. By Charles F. Delzell 454
- MARTA PETRICIOLI. *L'Italia in Asia Minore: Equilibrio mediterraneo e ambizioni imperialiste alla vigilia della prima guerra mondiale*. By John L. Harper 455
- ROBERT BLOBAUM. *Feliks Dzierżyński and the SDKPiL: A Study of the Origins of Polish Communism*.
By Joan S. Skurnowicz 455
- SHMUEL KRAKOWSKI. *The War of the Doomed: Jewish Armed Resistance in Poland, 1942-1944*.
By Stephan M. Horak 456
- KARL NEHRING. *Adam Freiherr zu Herbersteins Gesandtschaftsreise nach Konstantinopel: Ein Beitrag zum Frieden von Zsitvatorok (1606)*. By Marsha Frey 457
- DOMOKOS KOSÁRY. *Művelődés a XVIII. századi Magyarországon* [Culture in Eighteenth-Century Hungary].
By Andras Boros-Kazai 457
- MÁRIA ORMOS. *Padovától Trianonig, 1918-1920* [From Padua to Trianon, 1918-1920]. By Peter Pastor 458
- ALICE TEICHOVA and P. L. COTTRELL, editors. *International Business and Central Europe, 1918-1939*.
By John Komlos 459
- NARCISA LENGEL-KRIZMAN. *Zagreb u NOB-u* [Zagreb and the National Liberation Struggle]. By George J. Prpić 460
- JOHN F. CADZOW et al., editors. *Transylvania: The Roots of Ethnic Conflict*. By Gerald J. Bobango 460
- BARBARA JELAVICH. *Russia and the Formation of the Romanian National State, 1821-1878*. By John C. Campbell 461
- PROCOPIUS PAPAISTRATIS. *British Policy towards Greece during the Second World War, 1941-1944*. By Sidney Aster 462
- PAUL R. GREGORY. *Russian National Income, 1885-1913*.
By Daniel Mulholland 463
- TIMOTHY E. O'CONNOR. *The Politics of Soviet Culture: Anatolii Lunacharskii*. By Robert Chadwell Williams 463
- BIANKA PEITROW. *Stalinismus, Sicherheit, Offensive: Das "Dritte Reich" in der Konzeption der sowjetischen Aussenpolitik, 1933-1941*. By Richard K. Debo 464

NEAR EAST

- WILLIAM J. GRISWOLD. *The Great Anatolian Rebellion, 1000-1020/1591-1611*; ADEL ALLOUCHE. *The Origins and*

- Development of the Ottoman-Safavid Conflict (906-962/1500-1555).* By Rudi Paul Lindner 464
- SURAIYA FAROQHI. *Towns and Townsmen of Ottoman Anatolia: Trade, Crafts, and Food Production in an Urban Setting, 1520-1650.* By Justin McCarthy 465
- L. CARL BROWN. *International Politics and the Middle East: Old Rules, Dangerous Game.* By Frank Tachau 466
- CLIVE LEATHERDALE. *Britain and Saudi Arabia, 1925-1939: The Imperial Oasis.* By Roger Adelson 467
- YEHOYADA HAIM. *Abandonment of Illusions: Zionist Political Attitudes toward Palestinian Arab Nationalism, 1936-1939.* By Kenneth W. Stein 468
- JOAN PETERS. *From Time Immemorial: The Origins of the Arab-Jewish Conflict over Palestine.* By Robert Olson 468

AFRICA

- ABOUL KACEM SAADALLAH. *Tārikh al-Jazā'ir al-Thaqāfi: Min al-Qarn al-cAshir ilā l-Rābic cAshara l-Hijrī (16-20 M.)* [A Cultural History of Algeria: From the Tenth to the Fourteenth Century after the Hijra (16th-20th Century A.D.)]. By Peter Von Sivers 469
- GERMAIN AYACHE. *Les origines de la guerre du Rif.* By James J. Cooke 470
- MICHAEL WATTS. *Silent Violence: Food, Famine, and Peasantry in Northern Nigeria.* By Jan Hogendorn 470
- HARRY A. GAILEY. *Clifford: Imperial Proconsul.* By John W. Cell 472
- FATIMA BABIKER MAHMOUD. *The Sudanese Bourgeoisie: Vanguard of Development?* By M. W. Daly 472
- ARTHUR KEPPEL-JONES. *Rhodes and Rhodesia: The White Conquest of Zimbabwe, 1884-1902.* By Harry A. Gailey 473
- PETER WARWICK. *Black People and the South African War, 1899-1902.* By Richard H. Wilde 474

ASIA AND THE EAST

- RANAJIT GUHA. *Elementary Aspects of Peasant Insurgency in Colonial India.* By Anand A. Yang 474
- ASHIS NANDY. *The Intimate Enemy: Loss and Recovery of Self under Colonialism.* By Francis G. Hutchins 475
- WILLIAM C. KIRBY. *Germany and Republican China.* By Madeleine Chi 476
- LLOYD E. EASTMAN. *Seeds of Destruction: Nationalist China in War and Revolution, 1937-1949.* By Ernest P. Young 476
- JAY RUBIN. *Injurious to Public Morals: Writers and the Meiji State.* By Thomas M. Huber 477

UNITED STATES

- PHILIP F. GURA. *A Glimpse of Zion's Glory: Puritan Radicalism in New England, 1620-1660.* By Theodore Dwight Bozeman 478
- KENNETH SILVERMAN. *The Life and Times of Cotton Mather.* By Paul R. Lucas 478
- JON BUTLER. *The Huguenots in America: A Refugee People in New World Society.* By Stephanie Grauman Wolf 479
- BETTY WOOD. *Slavery in Colonial Georgia, 1730-1775.* By Daniel C. Littlefield 480
- WILLIAM E. FOLEY and C. DAVID RICE. *The First Chouteaus: River Barons of Early St. Louis.* By Gilbert C. Din 481

- E. WAYNE CARP. *To Starve the Army at Pleasure: Continental Army Administration and American Political Culture, 1775-1783.* By Paul David Nelson 482
- GARRY WILLS. *Cincinnatus: George Washington and the Enlightenment.* By John William Ward 482
- RALPH KETCHAM. *Presidents above Party: The First American Presidency, 1789-1829.* By Robert P. Hay 483
- PAUL R. PASKOFF. *Industrial Evolution: Organization, Structure, and Growth of the Pennsylvania Iron Industry, 1750-1860.* By Peter Temin 484
- G. EMLIN HALL. *Four Leagues of Pecos: A Legal History of the Pecos Grant, 1800-1933.* By Alvin R. Sunseri 484
- JOHN R. FINGER. *The Eastern Band of Cherokees, 1819-1900.* By Daniel F. Littlefield, Jr. 485
- RICHARD WHITE. *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos.* By James A. Clifton 486
- FREDERICK E. HOXIE. *A Final Promise: The Campaign to Assimilate the Indians, 1880-1920.* By Hazel Whitman Hertzberg 487
- MICHAEL BIRKNER. *Samuel L. Southard: Jeffersonian Whig.* By John M. Belohlavek 487
- LILIAN HANDLIN. *George Bancroft: The Intellectual as Democrat.* By Arthur A. Ekirch, Jr. 488
- RICHARD LEBEAUX. *Thoreau's Seasons.* By Fred Somkin 488
- JANE TURNER CENSER. *North Carolina Planters and Their Children.* By Crandall A. Shifflett 489
- KENNETH F. KIPLE and VIRGINIA HIMMELSTEIB KING. *Another Dimension to the Black Diaspora: Diet, Disease, and Racism.* By Allan H. Spear 490
- DALE BAUM. *The Civil War Party System: The Case of Massachusetts, 1848-1876.* By Jean H. Baker 491
- WILLIAM J. COOPER, JR. *Liberty and Slavery: Southern Politics to 1860.* By Paul H. Bergeron 491
- JAMES W. DADDYSMAN. *The Matamoros Trade: Confederate Commerce, Diplomacy, and Intrigue.* By Kinley Brauer 492
- GEORGE C. RABLE. *But There Was No Peace: The Role of Violence in the Politics of Reconstruction.* By Peyton McCrary 493
- MICHAEL PERMAN. *The Road to Redemption: Southern Politics, 1869-1879.* By Joe Gray Taylor 493
- NORMAN B. WILKINSON. *Lammot Du Pont and the American Explosives Industry, 1850-1884.* By Elisha P. Douglass 494
- HOWARD M. FEINSTEIN. *Becoming William James.* By Dorothy Ross 495
- PETER B. HALES. *Silver Cities: The Photography of American Urbanization, 1839-1915.* By Gunther Barth 496
- PERRY R. DUIS. *The Saloon: Public Drinking in Chicago and Boston, 1880-1920;* ROY ROSENZWEIG. *Eight Hours for What We Will: Workers and Leisure in an Industrial City, 1870-1920.* By John T. Cumbler 496
- DONNA R. GABACCIA. *From Sicily to Elizabeth Street: Housing and Social Change among Italian Immigrants, 1880-1930.* By John W. Briggs 497
- FERDINANDO FASCE. *Dal mestiere alla catena: Lavoro e controllo sociale in America (1877-1920).* By Andrew Rolle 498

JONATHAN DEMBO. <i>Unions and Politics in Washington State, 1885-1935</i> ; ALBERT F. GUNNS. <i>Civil Liberties in Crisis: The Pacific Northwest, 1917-1940</i> . By Carlos A. Schwantes	499	ROGER E. BILSTEIN. <i>Flight Patterns: Trends of Aeronautical Development in the United States, 1918-1929</i> . By Robert Frank Futrell	513
LEROY ASHBY. <i>Saving the Waifs: Reformers and Dependent Children, 1890-1917</i> . By John D. Buenker	499	JOHN F. SHINER. <i>Foulois and the U.S. Army Air Corps, 1931-1935</i> . By Robin Higham	513
JAMES E. DEVRIES. <i>Race and Kinship in a Midwestern Town: The Black Experience in Monroe, Michigan, 1900-1915</i> . By James Borchert	500	KENNY A. FRANKS. <i>Citizen Soldiers: Oklahoma's National Guard</i> . By John K. Mahon	514
SAM BASS WARNER, JR. <i>Province of Reason</i> . By Robert M. Crunden	501	RICHARD GID POWERS. <i>G-Men: Hoover's FBI in American Popular Culture</i> . By Daniel J. Czitrom	514
HERMANN R. MUELDER. <i>Missionaries and Muckrakers: The First Hundred Years of Knox College</i> . By Brooks Mather Kelley	501	WILLIAM S. BORDEN. <i>The Pacific Alliance: United States Foreign Economic Policy and Japanese Trade Recovery, 1947-1955</i> . By Akira Iriye	515
DONALD W. CURL. <i>Mizner's Florida: American Resort Architecture</i> . By Thomas S. Hines	502	ROBERT J. DONOVAN. <i>Tumultuous Years: The Presidency of Harry S. Truman, 1949-1953</i> . By Norman Markowitz	516
DOLORES HAYDEN. <i>Redesigning the American Dream: The Future of Housing, Work, and Family Life</i> . By Ruth Schwartz Cowan	503	ROGER BELL. <i>Last among Equals: Hawaiian Statehood and American Politics</i> . By Gary W. Reichard	517
CHRISTOPHER SILVER. <i>Twentieth-Century Richmond: Planning, Politics, and Race</i> . By David R. Johnson	504	DAVID R. JOHNSON et al., editors. <i>The Politics of San Antonio: Community, Progress, and Power</i> . By Kenneth Fox	517
NORMAN D. BROWN. <i>Hood, Bonnet, and Little Brown Jug: Texas Politics, 1921-1928</i> . By Roger M. Olien	504		
KENNETH R. MANNING. <i>Black Apollo of Science: The Life of Ernest Everett Just</i> . By Frederick M. Binder	505	CANADA	
MICHAEL W. HOMEL. <i>Down from Equality: Black Chicagoans and the Public Schools, 1920-41</i> . By Raymond Wolters	506	BARRY M. GOUGH. <i>Gunboat Frontier: British Maritime Authority and Northwest Coast Indians, 1846-90</i> . By Dwight L. Smith	518
DAVID TYACK et al. <i>Public Schools in Hard Times: The Great Depression and Recent Years</i> . By Ellen Condliffe Lagemann	506		
ROBERT S. MCELVAINE. <i>The Great Depression: America, 1929-1941</i> . By Richard Weiss	507	LATIN AMERICA	
JULIA KIRK BLACKWELDER. <i>Women of the Depression: Caste and Culture in San Antonio, 1929-1939</i> . By Alice Kessler-Harris	508	JEROME A. OFFNER. <i>Law and Politics in Aztec Mexico</i> . By Miguel León-Portilla	519
ROBERT E. SNYDER. <i>Cotton Crisis</i> . By Donald H. Grubbs	509	NANCY M. FARRISS. <i>Maya Society under Colonial Rule: The Collective Enterprise of Survival</i> . By Joseph W. Whitecotton	519
RICHARD LOWITT. <i>The New Deal and the West</i> . By Donald R. McCoy	509	MICHAEL C. MEYER. <i>Water in the Hispanic Southwest: A Social and Legal History, 1550-1850</i> . By Clifton B. Kroeber	520
JOAN HOFF-WILSON and MARJORIE LIGHTMAN, editors. <i>Without Precedent: The Life and Career of Eleanor Roosevelt</i> . By Linda J. Lear	510	CLIFTON B. KROEBER. <i>Man, Land, and Water: Mexico's Farmlands Irrigation Politics, 1885-1911</i> . By Michael C. Meyer	521
PHILIPPA STRUM. <i>Louis D. Brandeis: Justice for the People</i> . By Michael E. Parrish	510	DOUGLAS W. RICHMOND. <i>Venustiano Carranza's Nationalist Struggle, 1893-1920</i> . By Stanley R. Ross	521
MARK SILVERSTEIN. <i>Constitutional Faiths: Felix Frankfurter, Hugo Black, and the Process of Judicial Decision Making</i> . By Gerald T. Dunne	511	FRANCISCO A. SCARANO. <i>Sugar and Slavery in Puerto Rico: The Plantation Economy of Ponce, 1800-1850</i> . By Laird W. Bergad	522
HAROLD LEE. <i>Roswell Garst: A Biography</i> . By Gilbert C. Fite	512	RAYMOND CARR. <i>Puerto Rico: A Colonial Experiment</i> . By Louis A. Pérez, Jr.	523
		BARBARA WEINSTEIN. <i>The Amazon Rubber Boom, 1850-1920</i> . By Thomas H. Holloway	524
Collected Essays	525	Communications	543
Documents and Bibliographies	532	Index of Advertisers	34(a)
Other Books Received	536		

Order and Chaos in Early America: Political and Social Stability in Pre-Restoration Virginia

JON KUKLA

FOR TWO DECADES MOST AMERICAN HISTORIANS have supposed, as Jack P. Greene recently put it, that seventeenth-century Virginia was a "profoundly unstable world."¹ This mistaken impression, which has impeded a reinterpretation of the Chesapeake colonies and their place in early American history, gained ascendancy early in the 1960s. But the course of historiography might have been different. In 1958 Sigmund Diamond, standing on the solid scholarship of Wesley Frank Craven and Charles McLean Andrews,² pointed in the right direction when he asked how the Virginia Company outpost could have transformed itself into a genuine society without the emergence of social and political order in Virginia during the second quarter of the seventeenth century.³ Yet the ink of Diamond's essay was scarcely dry when Bernard Bailyn declared that "a veritable anarchy" existed in the seventeenth-century colony, which lacked "stable political alignments" and was generally "symptomatic of a profound disorganization of European society in its

I presented earlier versions of this essay at the 1979 meeting of the American Historical Association, at the 1983 meeting of the Organization of American Historians, and at the Philadelphia Center for Early American Studies seminar at Haverford College, January 18, 1985. Of the many historians, archivists, colleagues, and friends who have assisted my research, space allows mention only of my debts to Joyce Oldham Appleby, Lawrence Delbert Cress, Paul R. Lucas, Darrett B. Rutman, the editors of the *American Historical Review*, and, especially, Brent Tarter and Sandra Gioia Treadway for criticisms that improved various drafts of this essay.

¹ Greene, "Challenges to the Gentry," *Times Literary Supplement*, February 25, 1983, p. 177: "Especially over the past three decades, [historians] have emphasized the remarkable stability of [Virginia's] public life during the revolutionary era. . . . That conditions had not always been so has been made abundantly clear by several recent works on seventeenth-century Virginia and its newer Chesapeake neighbor, Maryland. Depicting a society that was crude, unhealthy, permissive, and marked by conflict, these studies have pointedly raised the questions of when and how the profoundly unstable world of seventeenth-century Virginia was transformed into the vastly more settled world of Washington's generation."

² Historians sometimes forget that Craven's *Dissolution of the Virginia Company: The Failure of a Colonial Experiment* (New York, 1932) and volume 1 of Andrews's *Colonial Period of American History: The Settlements*, 4 vols. (New Haven, 1934-38) were written before Susan Myra Kingsbury completed the compilation of the records of the Virginia Company. See Kingsbury, ed., *Records of the Virginia Company of London* [hereafter, *Virginia Company Records*], 4 vols. (Washington, D.C., 1906-34). Andrews's account of Virginia is at times unreliable; *Colonial Period of American History*, 1: 193-205. Craven's fifty-year-old classic remains as fresh as though it had been published yesterday.

³ Diamond, "From Organization to Society: Virginia in the Seventeenth Century," *American Journal of Sociology*, 63 (1957-58): 457-75.

American setting.”⁴ Although a few scholars have affirmed Diamond’s working hypothesis during the past two decades, Bailyn’s vision of anarchy and chaos was solidified into orthodoxy during the 1960s and 1970s without the support of much empirical data. Indeed, many young scholars uncritically accepted his interpretation despite their own evidence.

Evidence and synthesis moving in opposite directions—how could this be? Bailyn’s brilliant essay—each confident stroke sketching part of a vivid picture of Virginia society—captivated a generation of historians. It provided them with a convenient social background for Bacon’s Rebellion, a tidy explanation for the emergence of Virginia’s eighteenth-century landed gentry, and a welcome release from the burdens of research in difficult and fragmentary seventeenth-century manuscripts. Moreover, as an effort to reveal the “configuration of forces which shaped the origins of American politics,” the essay neatly paralleled aspects of Bailyn’s account of political development in seventeenth-century New England. When he and Gordon Wood carried the argument through the eighteenth century, Bailyn’s essay on Virginia became the preface to a sweeping synthesis that explained the origins of American politics, the ideological origins of the American Revolution, and the creation of the American republic.⁵ In a 1966 historiographical survey read by scores of young historians preparing for doctoral examinations, Greene coined the phrase “chaotic factionalism” and codified Bailyn’s view of early Virginia as an interpretive category for early American politics.⁶ Finally, in an exchange published by this journal in 1969, Bailyn and Greene tacitly concurred that it was pointless to look for the origins of American politics anywhere on this side of the Atlantic before the 1680s.⁷

Where historians of the stature of Bailyn and Greene found “a profoundly unstable world,” few others have thought to look for pattern, much less order. If we are to arrive at fresh insights about early America, we must ask whether this orthodoxy of chaos is really one of the “important themes . . . emerging in the work of the Chesapeake” or merely an unexamined assumption masquerading as a conclusion.⁸

Horror stories about the Starving Time at Jamestown in 1609–10 have prejudiced the image of early Virginia in the minds of historians. And yet the few who have closely studied the extant pre-Restoration records have described—in works published both before and after Bailyn’s essay—the emergence of social order within a few decades after that catastrophic winter. In a gem of a booklet written to

⁴ Bailyn, “Politics and Social Structure in Virginia,” in James Morton Smith, ed., *Seventeenth-Century America: Essays in Colonial History* (Chapel Hill, 1959), 90–115.

⁵ Bailyn, *New England Merchants in the Seventeenth Century* (Cambridge, Mass., 1955), *The Origin of American Politics* (New York, 1969), *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967); and Wood, *The Creation of the American Republic* (Chapel Hill, 1967).

⁶ Greene, “Changing Interpretations of Early American Politics,” in Ray Allen Billington, ed., *Reinterpretation of Early American History: Essays in Honor of John Edwin Pomfret* (San Marino, Calif., 1966), 167–77.

⁷ Greene, “Political Mimesis: A Consideration of the Historical and Cultural Roots of Legislative Behavior in the British Colonies in the Eighteenth Century,” *AHR*, 75 (1969): 337–60; Bailyn, “A Comment,” *ibid.*, 361–63; and Greene, “Reply,” *ibid.*, 364–67. Also see John M. Murrin, “Political Development,” in Jack P. Greene and J. R. Pole, eds., *Colonial British America: Essays in the New History of the Early Modern Era* (Baltimore, 1984), 408–56.

⁸ Thad W. Tate and David L. Ammerman, eds., *The Chesapeake in the Seventeenth Century: Essays in Anglo-American Society* (Chapel Hill, 1979), viii.

commemorate Jamestown's three hundred fiftieth anniversary, Wilcomb E. Washburn depicted a cohesive society and polity in the colony during the second quarter of the seventeenth century. Edmund S. Morgan also reported that by mid-century Virginia's society had become increasingly complex, that the colonists were living "more comfortably than most men did in England," and that they had begun "to look upon their raw new land as a home rather than a temporary stopping place."⁹

By 1630 at Littleton, a few miles downstream from Jamestown, George and Mary Menefie had an orchard of apple, cherry, peach, and pear trees, a garden graced with "the fruits of Holland and the roses of Provence." Nearby, at Mathews Manor, Samuel and Frances Mathews enjoyed "a fine house, and all things answerable to it." William and Joane Peirce, owners of plantations in Warwick and James City counties, kept a townhouse in the capital with a four-acre garden from which in 1629 they "gathered near 100 bushells of excellent figs." Mrs. Peirce believed that "of her own provision she could keep a better house in Virginia than in London for three or four hundred pounds a year."¹⁰ Rude makeshift shelters had long since given way to a range of more sophisticated structures, including both "fairly substantial brick- and timber-framed dwellings" and large earthfast houses.¹¹

By 1630 Virginians were numerous and their communities, homes, and government relatively stable. In 1622 Opechancanough's warriors killed some three hundred fifty colonists (including six members of the Council of State); twenty-two years later five hundred residents of outlying settlements died in the old chief's last

⁹ Washburn, *Virginia under Charles I and Cromwell, 1625-1660* (Williamsburg, Va., 1957); and Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 143. Also see Charles E. Hatch, *First Seventeen Years: Virginia, 1607-1624* (Williamsburg, Va., 1957), 110-11.

¹⁰ William M. Kelso, *Kingsmill Plantations, 1619-1800: Archaeology of Country Life in Colonial Virginia* (Orlando, Fla., 1984); Lothrop Withington, "Virginia Gleanings in England," *Virginia Magazine of History and Biography* [hereafter, *VMHB*], 14 (1906-07): 421-22; Minnie G. Cook, "Governor Samuel Mathews, Junior," *William and Mary Quarterly* [hereafter, *WMQ*], 2d ser., 14 (1934): 108-09; and W. G. Stanard, "Abstracts of Virginia Land Patents," *VMHB*, 1 (1893-94): 447. The Menefies also kept a house in Jamestown; they later moved to Buckland in Charles City County near Westover. Mrs. Mathews, a daughter of Sir Thomas Hinton, outlived two husbands in Virginia, John West and Abraham Piersey, before marrying Samuel Mathews, Sr., about 1629. In March and April 1984, David K. Hazzard and Nicholas M. Luccetti of the Virginia Research Center for Archaeology excavated Causey's Care (near Shirley in Charles City County), the early seventeenth-century plantation of Walter Aston (1607-56). The site, which was subsequently destroyed, included a barn and dairy, a brick privy, and a brick-lined cellar or warehouse. The excavations revealed some evidence of industry. Artifacts, such as brass candlesticks, gilded spurs, a ceramic doll's head, and a special iron for ironing ruffles, were those of a provincial planter-merchant enjoying a comfortable existence. Documentary evidence suggests that Aston was the Virginia factor in a family-based commercial network and that he was a partner of George Menefie. Aston's brother Robert imported at least 625 pounds of Virginia tobacco through the port of London in 1630 and at least 1,000 pounds in 1640. Also in 1640, Charles and John Aston (probably nephews or cousins) imported 1,944 pounds of tobacco. Public Record Office, London, State Papers, Domestic 16/371, Letters and Papers, 1637, no. 101; Public Record Office, London, Exchequer 190/34/4, Port book, Port of London, 1629-30, and Exchequer 190/43/5, Port book, Port of London, 1639-40; Jon Kukla, "Walter Aston of Causey's Care" (unpublished paper, April 5, 1984, filed at Virginia Research Center for Archaeology, Yorktown); and Virginia Historical Society, Richmond, William Byrd (1674-1744), Title book, 1637-1743, MS. 5:9 B9965:1, pp. 270-83. The archaeological discoveries at Causey's Care—especially the possibility of a warehouse, manufacturing site, and trade goods—suggest the Virginia headquarters of a family-based commercial network similar to that described by Neville Williams. See his "The Tribulations of John Bland, Merchant: London, Seville, Jamestown, Tangier, 1643-1680," *VMHB*, 72 (1964): 19-41.

¹¹ Cary Carson et al., "Impermanent Architecture in the Southern American Colonies," *Winterthur Portfolio: A Journal of American Material Culture*, 16 (1981): 135-96. Carson and his coauthors vigorously challenged Morgan's depiction of seventeenth-century Virginia in *American Slavery, American Freedom*. "The artifacts recovered from sites of the sixteen teens and twenties do not bear witness to Virginia's sottish ways and tawdry vices. . . . Morgan's boom-town hypothesis lays a false scent for the period as a whole." *Ibid.*, 163. Also see Ivor Noël Hume, *Martin's Hundred* (New York, 1982); and Kelso, *Kingsmill Plantations*.

attack. The first attack cost the colony 20 percent of its settlers, the latter only 6 percent. Mean life expectancies remained low throughout the colonial period, but these statistics can obscure other realities.¹² Seasoned colonists (immigrants who survived the first year's exposure to the new climate) grappled not with famine and anarchy but with questions amenable to political resolution: How best to administer justice, restrain unfair tobacco monopolists, support the church, secure legal titles to lands, keep the pigs out of the corn, keep unmarried indentured servants out of the hay? Through regular meetings of the General Assembly, local courts, and vestries—as well as the “thrusting out” of Governor John Harvey in 1635—the Virginia colonists forged a civilian political community and discovered the objectives toward which political action might be directed.

Disorder may have plagued early Maryland during the “plundering time” of the 1640s and 1650s and England and Scotland during the Civil Wars,¹³ but things appear to have gone differently in Virginia, as James Russell Perry has shown. Working with the only substantially complete body of Virginia local records for the pre-Restoration period, Perry methodically plotted recorded contacts between landowners on Virginia's Eastern Shore (modern Accomack and Northampton counties) between 1615 and 1655. From this extraordinarily detailed record of the day-to-day lives of the shore residents, he painstakingly reconstructed “the densely interconnected societal network [that] combined with recognized authority to produce a locally-felt sense of community.” Perry discovered and convincingly documented “a social climate of stability” on the Eastern Shore of Virginia before 1655. His conclusion was carefully understated: “for an area settled primarily by individuals, without an original communal goal . . . this was quite an achievement.”¹⁴ Additional detailed community studies such as Perry's are needed, but already it is evident that, although seventeenth-century Virginia was not Sir Lewis Namier's England, neither was it Thomas Hobbes's Nature.¹⁵

¹² William L. Shea, *The Virginia Militia in the Seventeenth Century* (Baton Rouge, La., 1983), 59–61. Male immigrants born in the early seventeenth century appear to have lived “considerably longer (seven and more years) than those born in the 1650s . . . while males born in the region and achieving age 20 had on average lifespans two and three years longer than their sons and grandsons”; Darrett B. Rutman and Anita H. Rutman, *A Place in Time: Explicatus* (New York, 1984), 53.

¹³ Russell R. Menard, “Maryland's ‘Time of Troubles’: Sources of Political Disorder in Early St. Mary's,” *Maryland Historical Magazine*, 76 (1981): 124–40. On the achievement of stability in Maryland, see Lois Green Carr and David W. Jordan, *Maryland's Revolution of Government, 1689–1692* (Ithaca, 1974); Lois Green Carr, “Sources of Political Stability and Upheaval in Seventeenth-Century Maryland,” *Maryland Historical Magazine*, 79 (1984): 44–70, “The Foundations of Social Order: Local Government in Colonial Maryland,” in Bruce C. Daniels, ed., *Town and Country: Essays on the Structure of Local Government in the American Colonies* (Middletown, Conn., 1978), 72–110; and Lorena S. Walsh, “Charles County, Maryland, 1658–1705: A Study of Chesapeake Social and Political Structure” (Ph.D. dissertation, Michigan State University, 1977). In any comparison of community institutions in Virginia and Maryland during most of the seventeenth century, we need to remember that Maryland had no parishes until after 1692; Michael Graham, “Churching the Unchurched: The Establishment in Maryland, 1692–1724,” paper presented at the Third Hall of Records Conference on Maryland History, held in St. Mary's City, May 17–20, 1984.

¹⁴ Perry, “The Formation of a Society on Virginia's Eastern Shore, 1615–1655” (Ph.D. dissertation, Johns Hopkins University, 1980), iv. Also see Joseph Douglas Deal III, “Race and Class in Colonial Virginia: Indians, Englishmen, and Africans on the Eastern Shore during the Seventeenth Century” (Ph.D. dissertation, University of Rochester, 1981), 110–11, 162–66.

¹⁵ For an attempt to make place of birth an explanation for a transition from the presumed anarchy of the seventeenth century to the golden age of Chesapeake culture, see David W. Jordan, “Political Stability and the Emergence of a Native Elite in Maryland,” in Tate and Ammerman, *The Chesapeake in the Seventeenth Century*,

BEFORE WE ATTEMPT TO RESOLVE the contradiction between the orthodoxy established by eminent scholars and the conclusions reached by diligent archival researchers, two questions are worth asking: just what does "stability" mean and how does it come about? These are problems of more than antiquarian interest to much of the modern world. In our search for answers, three opportunities for misconception must be avoided. First, the concept of stability must be used as an analytic category rather than as a term of approbation. Scholars engaged in explaining or advocating the development of stable, open, democratic systems—"consociational" polities—often forget the alternative possibility of stable, imposed, authoritarian systems—"control" regimes. Historical situations come readily to mind in which social and political order might have been preferable to violent upheaval, but stable societies and regimes whose repressive and vicious features no responsible scholar celebrates are also plentiful.¹⁶ Bailyn's reliance in "Politics and Social Structure in Virginia" on a landed-gentry, "consociational" model of stability (a comparative framework underscored by references to Namier's England) obscures rather than illuminates social and political developments in the booming mercantile and military world of an American tobacco colony at war with its Indian neighbors.¹⁷

A second trap is the unthinking assumption that social order must precede, or

243–73; Carole Shammas, "English-Born and Creole Elites in Turn-of-the-Century Virginia," *ibid.*, 274–96; Martin H. Quitt, "The Virginia House of Burgesses, 1660–1706: The Social, Educational, and Economic Bases of Political Power" (Ph.D. dissertation, Washington University, 1970); and John C. Rainbolt, "The Alteration in the Relationship Between Leadership and Constituents in Virginia, 1660 to 1720," *WMQ*, 3d ser., 27 (1970): 411–34. Among the historians of Chesapeake society who use quantitative methods, only Russell Menard has extended his work on the Chesapeake tobacco economy into the first half of the seventeenth century. For a good bibliography of Menard's work and related studies, see his "The Tobacco Industry in the Chesapeake Colonies, 1617–1730: An Interpretation," *Research in Economic History: A Research Annual*, 5 (1980): 109–77. Most historians examine the period after 1650 because older Virginia records do not attract historians interested in statistical analysis. Gaps in the earlier documents make quantification difficult. The worst destruction of records by fire or war between 1676 and 1865 occurred precisely in the Virginia localities that were settled earliest and most densely. Therefore, a study such as Darrett B. Rutman and Anita H. Rutman's *A Place in Time: Middlesex County, Virginia, 1650–1750* (New York, 1984) necessarily treats a locality that was part of the frontier during much of the seventeenth century. Nevertheless, the Rutmans' work on Middlesex strongly supports my argument for stability in early Virginia. "The combined phenomena of parental loss and the remarriage of surviving parents tended over time to result in ever more mixed and complex families in Middlesex. . . . Viewing such families, we could easily leap to the conclusion that childhood in this Chesapeake society was a matter of disruption and trauma and that family attachments were consequently weak. . . . Such a leap would neglect the very essence of this Chesapeake society. The family within its own house was basic, true, but the boundary between the immediate family (so paramount in our twentieth century) and a larger collectivity of kin and friends—the collective family of the neighborhood—was slim, permeable. And if the former was fragile, the latter was not. In this situation, stability for children as well as for adults lay not so much in the transitory family of the household but in the permanent network of friends and relations within which the family was embedded." Rutman and Rutman, *Place in Time: Middlesex County, Virginia*, 118–20. Demographic patterns based on Maryland sources may or may not be representative of Virginia in all respects, and statistics on mortality rates or life expectancies (no matter how important to the understanding of a past society) cannot in themselves be used to describe social organization or disorganization, stability or instability.

¹⁶ Ian Lustick, "Stability in Deeply Divided Societies: Consociationalism versus Control," *World Politics*, 31 (1978–79): 325–44; and Bailyn, "Politics and Social Structure in Virginia," 107–08, *Origins of American Politics*, 26. Political scientists have studied the requirements for political stability in connection both with nation building in Africa and the Third World and with ethnic or religious divisions in such countries as the Netherlands and Lebanon. See Adriano Pappalardo, "The Conditions for Consociational Democracy: A Logical and Empirical Critique," *European Journal of Political Research*, 9 (1981): 365–90; and G. Bingham Powell, Jr., *Contemporary Democracies: Participation, Stability, and Violence* (Cambridge, Mass., 1982), 212–28.

¹⁷ Bailyn, "Politics and Social Structure in Virginia," 107–08, *Origins of American Politics*, 26.

develop concurrently with, political stability. The reverse is probably more nearly true in newly settled colonies or newly formed nations, where some minimal imposition of political order—such as the suppression of criminal violence—must occur before the emergence of any regularized pattern of social relationships. Thus, J. H. Plumb's succinct definition of political stability—"the acceptance by society of its political institutions, and of those classes of men or officials who control them"—has limited utility for American historians because it implicitly refers to a preexistent social order.¹⁸ If historians continue to assume that preindustrial Europe is the best historical model of a stable society,¹⁹ then the whole concept (except as a mythic vision of a world lost or escaped²⁰) is useless to students of North American immigrant populations, who share the plight of the Texan advised by the English grounds keeper that in order to landscape his ranch like a country house he must start with a five-hundred-year-old lawn. Indeed, England had not fitted the familiar Old World stereotype since the thirteenth century. With "a very considerable social mobility, based on wealth rather than blood, and with few strong and permanent barriers between occupational groups, town and country, and social strata," Alan Macfarlane wrote, England's social, economic, and legal systems were "in essence different" from those of the Continent. English colonists did not "shed their traditional social structure as they walked down the gang-plank into the promised land," he argued, but rather "took with them a system very different from that present in the rest of the world."²¹

A third snare is the ease with which one can lose sight of the countless individual acts and decisions comprising what Michael Dalton in 1622 called "the preventing justice," when he urged English justices of the peace "to prevent the breach of the Peace [by] wisely foreseeing and repressing the beginnings thereof."²² In so-called stable societies throughout history, rulers and ruling classes have had to exert themselves to maintain their positions or promote order. Plumb was right, despite the biological metaphor in the title of his book, that "stability becomes actual through the actions and decisions of men, as does revolution."²³ Stability is an achievement, not a growth. It is the result of individual and corporate human volition. One must, therefore, object to the unspoken assumption behind Bailyn's conclusion that, because "their political dominance was a continuous achievement," early Virginia's resident leaders were somehow exceptional, their polity unstable, or their authority illegitimate. Nobility and knighthood never hurt to be sure, but in

¹⁸ Plumb, *The Growth of Political Stability in England, 1675–1725* (London, 1967), 12.

¹⁹ The word "instability" has a common-sense utility when used by social historians, but, in practice, scholars speak less of "social stability" (except as antonymous to "social instability") than of discrete kinds of societies.

²⁰ For a penetrating critique of the nostalgic assumptions of many contemporary social historians, see Joyce Appleby, "Value and Society," in Greene and Pole, *Colonial British America*, 290–316.

²¹ Macfarlane, *The Origins of English Individualism: The Family, Property, and Social Transition* (New York, 1978), 164–65, 202. Commenting on the mythic dimension in recent scholarship on the preindustrial era, Joyce Appleby observed that "each cut of the historian's axe backward into the layers of the past proves that the roots of modern society are very deep, and the ordered world against which to project the disruptive forces of modernity retreats"; Appleby, *Economic Thought and Ideology in Seventeenth-Century England* (Princeton, 1978), 19.

²² Dalton, *The Country Justice, Containing the Practice of the Justices of the Peace Out of their Sessions* (London, 1622), 7.

²³ Plumb, *Growth of Political Stability in England*, 13.

early Virginia, as elsewhere, stability required leaders capable of command and statecraft, regardless of whether they had learned these arts at court, in battle, in trade, or in the study. By reading some specific situations in the colony too abstractly, overemphasizing the "common medieval heritage," and merging social and political leadership, Bailyn confounded Max Weber's useful distinctions between "legal" and "traditional" authority.²⁴ Legal authority derives from the formal legality of the leader's *office* (signified by the commission to the governor and councilors), is bolstered by an established impersonal order (the Virginia Company of London and the English crown), and is limited in its competence by the law. In Weber's conception of traditional authority, by contrast, legitimacy derives not from an impersonal sociopolitical order but from the *person* of a chief whose prerogative is limited only by precedent. Defined in these ways, attributes of traditional authority can always contribute to effective leadership, but legal authority already was the *sine qua non* of legitimacy in the seventeenth-century Anglo-American world.²⁵ Clearly, a definition of "political stability" could be a useful analytic concept for some New World settings. The immediate obstacle has been that, while both historians and political scientists share "a basic agreement that 'political stability' somehow means the absence of violence, governmental longevity, the absence of structural change, legitimacy, and effective decision-making," the concept itself has remained "an excellent illustration of . . . fuzziness and confusion."²⁶ Ian Lustick's definition, which I translate from the jargon of political science, avoids the three pitfalls described earlier: "Political stability is achieved when a population can expect that public transactions happen and will continue to happen within regular patterns and without illegal violence."²⁷

If we bear in mind that order can be achieved through control regimes as well as through consociational polities, recent research suggests a four-part political evolution of early Virginia. First came the disorder of the initial settlement. Then,

²⁴ Bailyn, "Politics and Social Structure in Virginia," 94. For example, it is important to recognize, as Bailyn did not, that Governor George Yeardley was the specific target when the planters requested (in 1620) a governor "cythar Noble or little lesse in Honor or Dower." This acid reference to contemporary ideals came from prominent and experienced colonists who could have been potential rivals for the appointment. The planters' petition for better men was a pointed request for a man better than Yeardley, that "right worthie Statesman—for his own profit"; Kingsbury, *Virginia Company Records*, 3: 231–32, 4: 37. John Rolfe had praised Yeardley's predecessors—Lord De La Warr, Sir Thomas Gates, and Sir Thomas Dale—as "good and sufficient men, as well of birth and quallyty to command," but Yeardley's personal conduct was the sort that makes *parvenu* a term of derision. See Rolfe, *A True Relation of the State of Virginia Lefte by Sir Thomas Dale Knight in May Last 1616* (Charlottesville, Va., 1971), 3; and J. Frederick Fausz and Jon Kukla, eds., "A Letter of Advice to the Governor of Virginia, 1624," *WMQ*, 3d ser., 39 (1977): 110 n. 22. Yeardley's braggadocio after being knighted amused London, and his rapacious pursuit of self-interest was notorious even among profiteering Virginians. See Norman Egbert McClure, ed., *The Letters of John Chamberlain*, 2 (Philadelphia, 1939), 188. George Thorpe and De La Warr's brothers were better men, as were Sir Francis Wyatt, Sir William Berkeley, Samuel Mathews, Sr., and William Claiborne. Wyatt "governe[d] like a Moyses, accepting no person, nor profit"; Kingsbury, *Virginia Company Records*, 4: 38, 1: 436, 440.

²⁵ Weber, *The Theory of Social and Economic Organization*, trans. Talcott Parsons and A. M. Henderson (New York, 1947), 152, 325–63. Also see Appleby, "Value and Society," and "Industrial Effetes," *Democracy: A Journal of Political Renewal and Radical Change*, 3 (1983): 96–103.

²⁶ Leon Hurwitz, "Contemporary Approaches to Political Stability," *Comparative Politics*, 5 (1972–73): 449, 463.

²⁷ Lustick wrote, "'Stability' or 'political stability' will refer to the continued operation of specific patterns of political behavior, apart from the illegal use of violence, accompanied by a general expectation among the attentive public that such patterns are likely to remain intact in the foreseeable future"; "Stability in Deeply Divided Societies," 325.

between 1612 and 1630, the military regime of Lord De La Warr and the councilor-commander oligarchy of the 1620s imposed a control regime on their colony.²⁸ Third, and in part simultaneously, between 1618 and 1646 the elements of a civilian consociational polity were created, and civil offices—councilors, burgesses, and commissioners of what became the counties—rather than militia rank increasingly became the legal basis for the colonial elite's domination of the economy and society. Finally, between 1635 and about 1646 the Virginians completed their transition from an imposed military regime to an enduring civilian polity. Virginia's achievement of a consociational stability by mid-century can be demonstrated by reference to seven specific characteristics of stability outlined by Greene in 1975: low levels of collective violence and civil disorder, the absence or muting of longstanding issues that deeply divide the nation, routine acceptance by the political society of existing institutions and leadership structures, regularized relations among branches and levels of government, low levels of turnover among leaders, orderly transfers of authority or leadership without serious disruption of the polity, and the reduction of factional strife to levels at which it is either unimportant or routine.²⁹ While religious and political passions swept their Anglo-American contemporaries into strife and bloodshed, Virginia's leaders were able to transform their potentially disruptive factions into a stable political system. Individual and collective acts of statecraft, not impersonal forces, made that happen.

NEITHER OF THE ELIZABETHAN ANTECEDENTS to English colonization—the idea of creating a military stronghold for privateers and the dream of reproducing English landed society on plantations in Ireland—offered the Virginia Company of London a model for effective colonial government.³⁰ In 1607 the company naively put

²⁸ Lustick's interpretation assumes that the imposition of order in a control regime is the work of a group indigenous to the population, as was the case in early Virginia; *ibid.* Clearly, external forces contributed to instability on several occasions in pre-Restoration Maryland. Menard, "Maryland's 'Time of Troubles'"; and Carr, "Sources of Stability and Upheaval in Seventeenth-Century Maryland."

²⁹ "The widespread and largely uncritical use of the concepts *political instability* and *political stability* in recent literature on early American politics," Greene wrote, "underlines the need for some attempt at definition. . . . My contention, which will hopefully be refined by future discussion and consideration, is that for colonial America the concept of political stability may be used to characterize any situation in which the following seven conditions obtained: (1) ordinarily low levels of collective violence and civil disorder; (2) the absence or muting of longstanding issues that polarize or deeply divide the nation; (3) the routine acceptance by political society of existing institutional and leadership structures; (4) the regularization of relations among the several branches and levels of government; (5) low levels of turnover among leaders; (6) the orderly transfer of authority or leadership through constitutional challenges without serious disruption of the polity; and (7) the reduction of factional or party strife to levels at which it becomes either unimportant or no longer dysfunctional or routinized and functional. Political stability does not require: (1) social and/or political inertia; (2) complete public tranquility or the absence of rivalry and contention within the political system; [or] (3) permanency of membership in political factions or party groupings, a high degree of cohesiveness of [or?] solidarity among the political elite, or a monopoly of power by a single group within the elite." Greene, "The Growth of Political Stability: An Interpretation of Political Development in the Anglo-American Colonies, 1660–1760," in John Parker and Carol Urness, eds., *The American Revolution: A Heritage of Change* (Minneapolis, 1975), 32–33 n. 17. I have used Greene's criteria for political stability, but I do not agree with his contention that Virginia "is the most graphic example" of the "generally similar pattern of . . . drastic, almost chronic, political disorder and flux . . . through the first decades of the eighteenth century." *Ibid.*, 32–33.

³⁰ David B. Quinn, "Sir Thomas Smith (1513–1577) and the Beginnings of English Colonial Theory," *Proceedings of the American Philosophical Society*, 89 (1945): 543–60; and Howard Mumford Jones, "Origins of the Colonial Idea in England," *ibid.*, 85 (1942): 448–65.

Jamestown in the hands of a council (similar to a board of directors) and implored it to be "all of one mind for the Good of your Country and your own." The result, as Englishmen steeped in Aristotelian political science should have anticipated, was disastrous. "This *Plantacion* was gouerned by a *President & Councell Aristocratycallie* . . . above two yeres," John Rolfe reported in 1616, "in which tyme . . . envie, dissentions and jarrs were daily sowen amongst them."³¹

Under a new charter issued on May 23, 1609, the company gave charge of the colony to "one *able and absolute* governor" and imposed martial law. De La Warr, Sir Thomas Gates, and Sir Thomas Dale, soldier-governors chosen from the circle of English officers experienced in Ireland and the Low Countries, brought garrison government to Virginia, and military regimentation stabilized the colony's day-to-day existence. In 1618 the Virginia Company of London formally ended the military regime and offered English common law, new land tenure policies, and a general assembly to induce immigration, but an imposed militia-based stability continued to shape Virginia's development throughout the 1620s.³² The decade of Indian warfare after the Powhatan uprising of 1622 forged colony leaders, regardless of their social origins, into what J. Frederick Fausz called an "unlikely oligarchy" that dominated the other colonists, the neighboring Indians, and commerce in tobacco, maize, and furs. Beginning in September 1623 the councilors and commanders monopolized the entire Indian trade along the Chesapeake and its tributaries. Combining their civilian and military offices with commercial connections, the twenty-eight men who were members of the Virginia Council of State between 1622 and 1632 had powers to issue land patents, regulate indentured servants, set the prices of domestic and imported commodities, administer the estates of deceased colonists, direct the militia, determine Indian policy, control the colony's gunpowder, and appoint local militia officers. After the dissolution of the bankrupt Virginia Company in 1624 the oligarchs also became "guardians" for the

³¹ Philip L. Barbour, ed., *Jamestown Voyages under the First Charter, 1606-1609*, 1 (London, 1969), 46, 54; and Rolfe, *True Relation of the State of Virginia*, 3-4. For studies of the period of the Virginia Company, see Craven, *Dissolution of the Virginia Company*; and Alden Vaughan, *American Genesis: Captain John Smith and the Founding of Virginia* (Boston, 1975). Regarding the pervasiveness of Aristotelian concepts, Sir Robert Filmer lamented of his contemporaries that "what cannot be found in scripture, many do look for in Aristotle; for if there is any form of government besides monarchy, he is the man best able to tell what it is, and to let us know by what name to call it. . . . The usual terms in this age of aristocracy and democracy are taken up from him to express forms of government most different from monarchy: we must therefore make inquiry into Aristotle touching these two terms." Peter Laslett, ed., *Patriarcha and Other Political Works of Sir Robert Filmer* (Oxford, 1949), 187, 193. Also see Sir Thomas Smith, *De Republica Anglorum* (1st edn., London, 1565); Sir Walter Raleigh, *Maxims of State* (London, 1650); Lawrence V. Ryan, "Richard Hakluyt's Voyage into Aristotle," *Sixteenth Century Journal*, 12 (1983): 73-83; Charles B. Schmitt, *John Case and Aristotelianism in Renaissance England* (Montreal, 1983), 26-29, 59-76; Corinne Comstock Weston, *English Constitutional Theory and the House of Lords, 1556-1832* (New York, 1965), 1-43; Zera S. Fink, *The Classical Republicans: An Essay in the Recovery of a Pattern of Thought in Seventeenth-Century England* (Evanston, Ill., 1945), 2-10; W. D. J. Cargill Thompson, "The Philosopher of the 'Politie Society': Richard Hooker as a Political Thinker," in W. Speed Hill, ed., *Studies in Richard Hooker: Essays Preliminary to an Edition of His Works* (Cleveland, Ohio, 1972), 46; and Norman Fiering, *Moral Philosophy at Seventeenth-Century Harvard: A Discipline in Transition* (Chapel Hill, 1981), 62-103.

³² The Virginia Company of London wrote into its first charter (April 1606) and instructions (November 1606) provisions for the resident council. Samuel M. Bemis, ed., *Three Charters of the Virginia Company of London with Seven Related Documents, 1606-1621* (Williamsburg, Va., 1957), 1, 14; Darrett B. Rutman, "A Militant New World, 1607-1640. . . ." (Ph.D. dissertation, University of Virginia, 1959), "The Virginia Company and Its Military Regime," in Rutman, ed., *The Old Dominion: Essays for Thomas Perkins Abernethy* (Charlottesville, Va., 1964), 4-9, "The Historian and the Marshall: A Note on the Background of Sir Thomas Dale," *VMHB*, 68 (1960): 284-94; and Craven, *Dissolution of the Virginia Company*, 78-79, 94-97.

property and servants once owned by the company.³³ The councilor-commander oligarchy of the 1620s had no challengers. Charles I was preoccupied with other affairs. The unicameral General Assembly (founded mainly as a vehicle for the company to secure the colonists' support of company policies) was in many respects before 1643 only an expanded meeting of the governor and council. Not until the 1630s did Virginia's population growth give the councilors reason to delegate any of their consolidated military, executive, and judicial authorities.

Ignoring hard-liners who advocated "a perpetuall warre without peace or truce" to extirpate the Powhatans, Virginia's councilor elite devised a policy of limited Indian warfare that a contemporary compared to a greedy "Surgion" who treated a patient for "3 yeares, that maybe Cured in 3 quarters, or 3 monethes." Pressing colonists into service for biannual plundering raids (which they called "feedfights" and "harshe visits"), the elite captured a rich contraband of maize and furs that they then sold to the colonists for tobacco. In a single expedition against the Pamunkeys in 1622, George Yeardley gathered more than a thousand bushels of corn worth five hundred to one thousand pounds sterling. Yeardley and the councilor-commander elite took "paynes to burne a few of [the Powhatans'] houses, everie yeare," but never totally destroyed their villages. The Virginians encouraged the Powhatans to return to the same fields year after year to plant, while they returned season after season for the harvest. The commanders thus controlled and exploited not only the indentured servants who comprised the vast majority of new immigrants in the 1620s but also, in Fausz's telling phrase, the "red peasants" who grew the corn harvested in seasonal raids.³⁴

These were the boom years. Tobacco production skyrocketed between 1616 and 1626: from 2,300 pounds in 1616 at an unknown market price, to 41,000 pounds in 1618 at three shillings the pound, to 60,000 four years later, and then to 260,254 pounds at about a shilling the pound in 1626. With a low production cost and exceedingly favorable English market, a colonist's investment of twenty pounds sterling might earn fifty or sixty pounds annually; tobacco culture promised an immediate return of 250 to 300 percent. In the long run, of course, as production increased and approached demand, this remarkable market changed, and the extraordinary profit margin disappeared. Through the mid-1620s, however, there were fortunes to be made in tobacco, and in the 1630s, when the unusual profitability of tobacco ended, adventurous investors such as William Claiborne

³³ Kingsbury, *Virginia Company Records*, 4: 37; and J. Frederick Fausz, "Coming of Age through Crisis: Virginia and the Unlikely Oligarchy, 1620–30," paper presented at the Ninety-Fourth Annual Meeting of the American Historical Association, held in New York, December 28–30, 1979, "Authority and Opportunity in the Early Chesapeake: The Bay Environment and the English Connection, 1620–1640," paper presented at the Seventy-Sixth Annual Meeting of the Organization of American Historians, held in Cincinnati, April 6–9, 1983, "Merging and Emerging Worlds: The Interplay of Anglo-Indian Interest Groups in the Early Chesapeake, 1620–1660," paper presented at the Third Hall of Records Conference on Maryland History, held in St. Mary's City, May 17–20, 1984.

³⁴ Fausz, "Coming of Age Through Crisis," "City, Shire, and Commonwealth: London, Kent, and the Emergence of English Virginia, 1580–1630" (National Endowment for the Humanities Seminar paper, Northwestern University, 1980), "Profits, Pelts, and Power: The 'Americanization' of English Culture in the Chesapeake, 1620–1652," paper presented at the Ninety-Seventh Annual Meeting of the American Historical Association, held in Washington, D.C., December 28–30, 1982; and Shea, *Virginia Militia in the Seventeenth Century*, 46–50.

exploited the similar pattern of profit margins then available in the upper Chesapeake fur trade. Tobacco remained profitable as prices fell, but the eroding rate of profit transformed Virginia. Established councilor-commander entrepreneurs continued to thrive (and even found it worthwhile to trade in commodities, such as fur, corn, cattle, and hogs), but lower rates of return on tobacco prevented new would-be entrepreneurs from making places for themselves. Irene Hecht has found that the percentage of Virginia's immigrants who were self-financed, the would-be entrepreneurs, fell from 90 to less than 10 percent between 1624 and 1637. In the 1630s those who made the journey—Robert Evelin's "long and dangerous voyage . . . to make me to be able to pay my debts, and to restore my decayed estate again"—found on arrival that a few good tobacco crops no longer brought a quick fortune.³⁵

By 1630 Virginia had a social spectrum with well-defined poles: at one extreme the councilor-commanders and at the other the smallest planters and indentured servants. Many questions still need answers. Precisely how many Virginians were at each pole? How extensive in numbers were planters of middling success? How did the indentured servants fare? How did slavery develop? But clearly by 1630 the small group of councilor-commanders had become a cluster of commerce-minded planters at the pinnacle of Virginia's society. Whatever their social origins, these men found common ground during the tobacco and maize boom of the 1620s. Gentry-born leaders rediscovered the practicality of a Renaissance courtier's military skills, while plain-born men found that habits of leadership learned in the Indian wars had civilian utility. Their camaraderie enforced by the fight against a common enemy, this enterprising group owned the ships and formed the partnerships that dominated the colony's internal and external trade in corn, fur, and tobacco. The virtually complete domination of Virginia by this single, cohesive group imposed order on a potentially unstable population of smaller planters and indentured servants whose share in the fortune, although smaller or deferred, was still real.

Three important things happened between 1630 and 1642 as the councilor-commander elite transformed its control regime into a consociational polity. First, with the end of constant Indian warfare in 1632 and the deaths of some older militia leaders, such as John Utie and William Tucker, civilian offices on the council, in the assembly, and in the emerging counties supplanted military offices as the primary locus of power in Virginia's society and economy. Second, in

³⁵ Hecht, "The Virginia Colony, 1607–1640: A Study in Frontier Growth" (Ph.D. dissertation, University of Washington, 1969); and Menard, "Tobacco Industry in the Chesapeake Colonies," 109–77. For Evelin's letter of 1610, see Alexander Brown, *The Genesis of the United States*. . . (Cambridge, Mass., 1897), 441–42. Also see T. H. Breen, ed., "George Donne's 'Virginia Reviewed': A 1639 Plan to Reform Colonial Society," *WMQ*, 3d ser., 30 (1973): 449–66. Historians have not yet investigated alternative staples in the Chesapeake colonies during the second quarter of the seventeenth century. Both David Klingaman and Paul G. E. Clemens focused on eighteenth-century agricultural changes. See Klingaman, "The Significance of Grain in the Development of the Tobacco Colonies," *Journal of Economic History*, 29 (1969): 268–78; and Clemens, *The Atlantic Economy and Colonial Maryland's Eastern Shore: From Tobacco to Grain* (Ithaca, 1980). Fausz's work shows the importance of the internal maize trade, and there is some evidence that Maurice Thompson and Samuel Mathews, Sr., were exporting maize and victuals to the Caribbean colonies in the middle decades of the seventeenth century. See Fausz, "The Powhatan Uprising of 1622: A Historical Study of Ethnocentrism and Cultural Conflict" (Ph.D. dissertation, College of William and Mary, 1977), 456–82; and Deal, "Race and Class in Colonial Virginia," 2–5.

this changed context the old elite consciously began acting as a civilian political group, becoming in the words of an opponent “growne upp to maturity and ripeness of faction.” Third, the faction (led by Samuel Mathews, Sr., “the head & cheefe support,” and by Claiborne) turned its energy away from the local maize monopolies of the Indian-war years and toward the creation of a transatlantic monopoly of the entire American tobacco trade.³⁶

Had not other dangers seemed immediate in the 1630s, the Mathews-Claiborne faction of wealthy merchant-planters might have alienated every other planter in the colony. But every potential source of conflict between governor and colonists seems to have flared up soon after Governor Harvey came to Virginia in 1630, and in 1632 Charles I added yet another by giving Virginia’s northern Chesapeake Bay area to Lord Baltimore and directing Harvey to assist the Virginians’ Roman Catholic rival. In the 1630s Maryland succeeded the Indians as the hated outside enemy that united Virginia colonists and obscured real differences that became more visible later. Perhaps an exceptional politician could have survived, but Governor Harvey insisted on greater prerogative authority than his commission specified, pressed the crown’s instructions with more zeal than effectiveness, and—with foolhardy thoroughness—angered every important Virginian in the colony.³⁷

Late in April 1635, after years of conflict, the council (dominated by the Mathews-Claiborne faction) put Harvey under arrest, elected John West (a younger brother of the late Lord De La Warr, a prominent councilor-commander since the 1620s, and Mathews’s brother-in-law) as interim governor, and summoned a meeting of the General Assembly for May. On May 23, the Virginians sent Harvey to England in the custody of Francis Pott and Thomas Harwood (both prominent councilor-commanders since the 1620s), who were to present the assembly’s charges against Harvey to the Lords Commissioners for Plantations (forerunner of the Board of Trade). The royal authorities had no real choice but to acquit Harvey and send him back to Virginia to reassert the authority of his office, but they did so in 1637 without enthusiasm and without bestowing any signs of favor. Harvey asked to be transported in a royal vessel as “an honor to the King’s governor” and a gesture to “check the boldness of the offenders in the Colony”; he got passage aboard the *Black George*, a craft so unseaworthy that he had to turn back and sail to Virginia aboard a commercial vessel.³⁸

³⁶ Breen, “George Donne’s ‘Virginia Reviewed,’” 458, 460; J. Mills Thornton, “The Thrusting Out of Governor Harvey: A Seventeenth-Century Rebellion,” *VMHB*, 76 (1968): 18–19; Public Record Office, London, Colonial Office [hereafter, CO] 1/8, ff. 106–07. For the classic study of efforts to manage the pre-Restoration tobacco trade, see George Louis Beer, *The Origins of the British Colonial System, 1578–1660* (New York, 1908), 110–73. For a description of the James River-to-London trade axis, for which the Mathews-Claiborne faction formed the western terminus, see Jon Kukla, “Political Institutions in Virginia, 1619–1660,” (Ph.D. dissertation, University of Toronto, 1979), chaps. 3, 4; Robert Paul Brenner, “Commercial Change and Political Conflict: The Merchant Community in Civil War London” (Ph.D. dissertation, Princeton University, 1970); and J. E. Farnell, “The Navigation Act of 1651, the First Dutch War, and the London Merchant Community,” *Economic History Review*, 16 (1963–64): 439–54.

³⁷ CO 1/8, ff. 197–98. For a printed text, with many minor transcription errors, see *VMHB*, 1 (1893–94): 425–30. Also see Warren M. Billings, ed., *Old Dominion in the Seventeenth Century: A Documentary History of Virginia, 1606–1689* (Chapel Hill, 1975), 254–57.

³⁸ For the constitutional issues and transatlantic political and mercantile context not treated in Thornton’s fine narrative of the removal of John Harvey, see Kukla, “Political Institutions in Virginia,” chap. 3. Peter C. Hoffer and N. E. H. Hull’s first account is muddled in many respects. See Hoffer and Hull, “The First

Despite his formal acquittal, Harvey had failed as governor; his Virginia enemies and their influential English friends had little trouble getting him recalled. Sir Francis Wyatt, serving again as governor from 1639 to 1641, calmed the political waters and began a program of political and judicial reform that enhanced the possibility for consociational stability. As governor from 1621 to 1626 Wyatt had guided the colony through the traumas of the Powhatan uprising of 1622 and the dissolution of the Virginia Company of London and had participated in the creation of the councilor-commander oligarchy. The scion of a prominent Kent family, Wyatt was a man of proven ability, familiar with Virginia, and respected by its inhabitants. He was returning to the colony after serving on the Dorset commission, the second of three groups that shaped royal policy toward Virginia after the dissolution of the Virginia Company. Wyatt had probably written two key provisions into his instructions: royal recognition of the General Assembly and confirmation of the land titles of the "present planters and possessors." As a result, the king's incoming governor ended fifteen years of uncertainty and transformed Virginia's political life.³⁹

Wyatt moved quickly to speed the administration of justice by the colony's overburdened court system. His reforms of 1639–43 made the county courts a bulwark of stability for centuries to come. Even in the best of times, planters from Henrico, Lower Norfolk, and the Eastern Shore could not troop to Jamestown to prove wills, settle small debts, punish drunkards and fornicators, pay taxes or tithes, and arrange the routine maintenance of local jails, ferries, and bridges. Furthermore, colony officials at Jamestown could not have handled such local tasks even for the capital's immediate environs. Between 1624 and 1634 the population of James City, Charles City, and Warwick counties increased from 647 to 2,208, and the colony's population rose from 1,227 in 1624 to 4,909 in 1634 and 10,442 in 1640. And the 1630s were not the best of times, for the market price of tobacco plummeted from thirteen pence per pound in 1624 to fivepence in the mid-1630s and threepence at the close of the decade. Population growth and a tightening economy accompanied a huge increase in the number of lawsuits in the colony's courts—a statistic that is the best available index of change in the burdens of general administration imposed on the existing political system. The number of actions in the Accomack County Court, for example, increased fivefold in the six years between 1632 and 1638, from 40 to 249, and more than 80 percent of these were debt cases.⁴⁰ In 1639 a proclamation from Wyatt and the council suspended

American Impeachments," *WMQ*, 3d ser., 35 (1978): 655–58. For their modified account, see Hoffer and Hull, *Impeachment in America, 1635–1805* (New Haven, 1984). Because the crown's commission granted authority to governor and councilors, a strong case should be made that the councilors' actions in removing Harvey were legal, although extreme. Compare Harvey's claim in 1635 that "only the Governor is of the Quorum" with his admission in 1634 to Secretary Windebanke that his authority was "not great, it being limited by my commission to the greater number of voyces at the counsell table"—a position to which Harvey and the councilors had committed themselves in a document of accord signed in 1631. CO 1/6, ff. 92–93, 1/8, ff. 106–07, 178–81, 197–98.

³⁹ Richard L. Morton, *Colonial Virginia* (Chapel Hill, 1960), 141–46; Charles I's commission to Wyatt, January 11, 1639, CO 5/1354, ff. 212–18; and Fausz and Kukla, "A Letter of Advice to the Governor of Virginia, 1624," 104–29. For a printed text of Wyatt's commission, see *VMHB*, 11 (1903–04): 50–54.

⁴⁰ Warren M. Billings, "The Growth of Political Institutions in Virginia, 1634 to 1676," *WMQ*, 3d ser., 31 (1974): 228–30; Carr, "The Foundations of Social Order"; Robert Wheeler, "The County Court in Colonial

settlement of debts in court, in 1640 the General Assembly passed several laws designed to relieve indebted colonists, and in 1641 the assembly continued Wyatt's reform of the judicial system. A year later, fifty-four-year-old Wyatt gave to his successor, thirty-four-year-old Sir William Berkeley, both the task of completing the reform and the opportunity to modify it. In this way, a judicial reform undertaken for "the better ease of the Country and quicker dispatch of business" set the stage for wider changes in the structure of Virginia government that have had lasting importance in American history.⁴¹

Reform legislation kept Wyatt and the assemblies of 1640, 1641, and 1642 working longer than usual. In 1641 their "well ordering and settling of many Weighty Affaires Controverted and Concluded" required so much "more tyme then was at first expected" that Wyatt postponed the March quarter session of the council for four weeks to allow "the Counsell and the Burgesses such as have causes depending to settle their domesticall Affaires and returne again." Wyatt had adjourned, not dissolved, the assembly session begun on January 12, 1642, and Berkeley twice reconvened the old members without conducting new elections. He did this to "prevent all doubts" about the "acts already agreed upon" and to avoid procedural objections about "the many and weighty business[es] begun in the present Grand assembly." Berkeley's month-long session of the General Assembly in June 1642 also exceeded the "customary limits of time," producing laws so "few in number" that the assembly issued an apologetic "Remonstrance" calling attention both to its judicial reforms and to other unspecified "benefits redounding" from their "consultations."⁴²

Only nine of the statutes of 1641 and none of the statutes of 1642 survive in manuscript, but five acts from 1641 and eleven from 1642 can be identified among the seventy-three acts passed in the revival of 1643, which has survived. This evidence discloses some of the judicial reforms, such as protecting officers of the law from suit for actions taken in performance of their duties, defining the local courts' role in handling land grants and orphans' estates, specifying meeting dates for the quarter court and rules of serving writs and arrests, affirming the right to jury trial when requested by either party in a civil case, and fixing a staggered schedule of local court days "for the convenience of those having business in more than one court." In sum, the June 1642 culmination of Wyatt and Berkeley's

Virginia," in Daniels, *Town and Country*, 111–13; and George B. Curtis, "The Beginnings of a County Court: Accomack County, Virginia, 1633–1639 (M.A. thesis, University of Virginia, 1971), "The Colonial County Court, Social Forum and Legislative Precedent: Accomack County, Virginia, 1633–1639," *VMHB*, 85 (1977): 274–88. For the proclamation of Wyatt and the council, June 16, 1642, see Virginia Historical Society, Richmond, Lower Norfolk County Court Records, 1637–43, vol. 1, 159 [hereafter, LNCCR]. Regarding interpretations of the early Virginia General Assembly, see Craven, "And so the Form of Government Became Perfect," *VMHB*, 77 (1969): 131–45; Jon Kukla, "Robert Beverley Assailed: Appellate Jurisdiction and the Problem of Bicameralism in Seventeenth-Century Virginia," *VMHB*, 88 (1980): 415–29; and Murrin, "Political Development," 450 n. 26.

⁴¹ Susie M. Ames, ed., *County Court Records of Accomack-Norhampton, Virginia, 1640–1645* (Charlottesville, Va., 1973), 99; and William Waller Hening, ed., *Statutes at Large: Being a Collection of All the Laws of Virginia, from the First Session of the Legislature in the Year 1619 . . .*, 1 (Richmond, Va., 1823), 236–38. Concerning the General Assembly of 1641, see Jon Kukla, ed., "Nine Acts of the Grand Assembly of Virginia, 1641" (unpublished typescripts filed at Virginia State Library, Richmond, and at Virginia Historical Society, Richmond, of untitled seventeenth-century document, accession no. 25719 of the Virginia State Library).

⁴² Hening, *Statutes at Large*, 236–38.

reform of the judiciary made it easier for colonists to conduct their business and for the magistracy, whether in the counties or at Jamestown, to contend with the legal affairs of a large and complex population.⁴³

And there was more. Clearly the assembly's Remonstrance of 1642 did not name all provisions of the reform legislation. The act extending trial by jury to civil cases, for example, appears to be one of the unspecified provisions covered by general references in the Remonstrance to "the birthright of [the] mother nation," "to the laws and customs of England in proceedings of the court and trials of causes," and to "the opportunity of establishing . . . liberties and privileges and settling . . . estates . . . and of preventing the future designs of monopolizers, contractors and preemptors." A substantial body of evidence also suggests that the introduction of bicameral procedure in the General Assembly was another of the unspecified "customs of England." At the next meeting of the assembly in March 1643, the elected members organized themselves, for the first time in any English colonial assembly, as a lower house meeting separately from the governor and councilors. Within a few years Virginia's bicameral legislature was so well known in the colonies that it was cited as the precedent for the "Politique and Civill Government" of a proposed colony in which the governor and councilors were to constitute "the Councill of State or upper House" and "thirty Burgesses, or Commons," comprise the lower house of a "parliament or Grand assembly."⁴⁴

FOR TWO DECADES AFTER 1619 THE UNICAMERAL STRUCTURE of Virginia's General Assembly had reflected a commonality of interest among the colony's small, geographically concise population and its leadership. The change to bicameralism corresponded with the suddenly enhanced visibility of rival interest groups in the colony's socioeconomic system.⁴⁵ No governor could survive the opposition of a unified populace led by the same men who had expelled Harvey, but Berkeley perceived that the Mathews-Claiborne faction's goal of a tobacco monopoly and the exclusion of Dutch traders was antithetical to the smaller planters' interest. Before the crown's formal recognition of land titles in 1639, most Virginians seemed willing to revive the Virginia Company of London and give it a tobacco-trade monopoly as a quid pro quo for firm titles to their lands. After 1639, however, the only men with reason to support reinstatement of the company were the prominent members of the Mathews-Claiborne faction and their allies in the circle of London merchants around Maurice Thompson, author of the Navigation Act of 1651. The specter of a revived company served Berkeley well, for he began his career in Virginia by crushing the idea so masterfully that historians cannot trace the efforts of those who had favored it. The "Declaration against the Company" adopted by Berkeley and the assembly of 1642 denounced the claim of George Sandys, the

⁴³ *Ibid.*

⁴⁴ *Ibid.*; Kukla, "Political Institutions in Virginia," chap. 4, *Speakers and Clerks of the Virginia House of Burgesses, 1643-1776* (Richmond, Va., 1981), 10-20; and *A Description of the Province of New Albion*, in Peter Force, ed., *Tracts and Other Papers*, 2 (Washington, D.C., 1836), 38.

⁴⁵ Kukla, "Political Institutions in Virginia," chap. 4; and J. R. Pole, *The Seventeenth Century: The Sources of Legislative Power* (Charlottesville, Va., 1969), 64.

spokesman for the elite, that the colonists uniformly favored a revived company. With land titles secure, Sandys spoke only for the same merchant-councilor faction that had ousted Harvey. Those who "with most secret reservation and most subtlety argue for a company," the Declaration charged, sought both "propriety to the land and power of managing the trade, which word managing [is but] a convertible [that is, synonym] to monopolizing." By threatening the "freedom of our trade," proponents of the revived company endangered "the blood and life of a commonwealth!"⁴⁶

Berkeley balanced the power of the Mathews-Claiborne faction not only with the new House of Burgesses but also by drawing together something of a gubernatorial faction among the elite. Outside the James River basin, especially on the Eastern Shore and in the vicinity of modern Norfolk, trade with Dutch merchants was extensive. Prominent colonists in these areas had remained individually independent of monopolists on the James River-to-London axis, for their interests in Dutch and non-London shipping put them at odds with the Mathews-Claiborne faction. Identification of the members of this group is necessarily fragmentary, for they lacked the well-defined economic goal of the James River monopolists. Berkeley's potential allies included such independent merchant-planters as Argoll Yeardley, Edmund Scarborough, John Custis, George Ludlow, Samuel Abbot, Ralph Wormeley, and Richard Lee.⁴⁷

Berkeley also turned crises to his advantage and used local government to maintain stability in Virginia while Maryland and England were in upheaval. Close examination of the specific authorities delegated to the counties in the 1640s contradicts the widely held belief that county court powers expanded at the expense of central authority.⁴⁸ Sheriffs were made responsible for collecting the fees of the colony secretary and the council clerk, for bringing defendants to the

⁴⁶ H. R. McIlwaine, ed., *Journals of the House of Burgesses of Virginia, 1619–1658/59* (Richmond, Va., 1915), 66–69; Henning, *Statutes at Large*, 230–38. Unfortunately, the best published account of this attempt to revive the Virginia Company is marred by a narrowly biographical focus and special pleading. See Richard Beale Davis, *George Sandys, Poet-Adventurer: A Study in Anglo-American Culture in the Seventeenth Century* (London, 1955), 261–65.

⁴⁷ A list of debts owed to Richard Glover, an English-born merchant trading out of Amsterdam, provides a partial rollcall of those who were outside the Mathews-Claiborne faction. Glover evidently did not trade with the Eastern Shore; only one person on Glover's list, Mary Menefie, George Menefie's widow and executrix, had any known link to Mathews and Claiborne. In addition to Mrs. Menefie and Governor Berkeley himself, Glover's list identifies the following persons as likely members of the group of independent merchant-planters: George Ludlow, who became a member of the council in 1642; Samuel Abbot, clerk of the council from 1643 to 1647; Bridges Freeman and Ralph Wormeley, who became members of the council in 1650; Richard Lee, who joined the council in 1651; Robert Holte, a burgess in 1656 and again from 1666 to 1676; Stephen Gillis, a burgess in 1652; and John Chew, a burgess at various times for two decades between 1624 and 1644. The manuscript containing the list is torn, which prevents us from learning the extent of Glover's trade with each of the twenty-one independent merchant-planters, but the average debt owed him was twenty-seven hundred pounds of tobacco per person. Virginia State Library, Richmond, York County Records No. 2, Wills and Deeds, 1645–49, p. 95. I am grateful to Warren M. Billings for permitting me to consult the lists of Dutch merchants engaged in the Virginia trade that he compiled during his systematic research in all the extant Virginia county records from 1634 to 1676; fifty of the fifty-seven Dutch merchants traded with the Eastern Shore. Also see John R. Pagan, "Dutch Maritime and Commercial Activity in Mid-Seventeenth-Century Virginia," *VMHB*, 90 (1982): 485–501. Pagan did not examine the interplay of trade connections and Virginia political alignments.

⁴⁸ Billings, "Growth of Political Institutions," *Old Dominion in the Seventeenth Century*, 68–81; and Jon Kukla, review of Billings's *Old Dominion in the Seventeenth Century*, in *VMHB*, 83 (1975): 479–80. For a description of an extreme measure of provincial action to control a wayward county official, see Susie M. Ames, "The Reunion of Two Virginia Counties," *Journal of Southern History*, 8 (1942): 536–48.

General Court, and for serving gubernatorial writs of execution. County clerks, appointed by the governor, were required to bring "a true copy of all [their] proceedings at every Quarter Court unto the Governor and Council" and to maintain "an exact register" of orphans' estates, chancery cases, and the like for review by the central authorities. Councilors were empowered to ride circuit and sit with the county justices as senior justices. By this shrewd delegation of specific powers Berkeley arranged for an effective local administration that reinforced his central authority. His enforcement of religious uniformity provides the best-documented example of these tactics in action.⁴⁹

That story began, circuitously, with the aged Opechancanough's last surprise attack on April 18, 1644. The loss of some five hundred colonists at outlying plantations on that Good Friday did not debilitate the colony but pulled the surviving ten thousand together. With the assembly's blessing and letters of credit, Berkeley sailed for England "to Implore his Majesty's gracious assistance for our Releife," while Claiborne, the arch-merchant-councilor himself, led the militia against Opechancanough. But, in a tactic reminiscent of his councilor-commander practices in the 1620s, Claiborne diverted the militia from its intended target to pursue his own lifelong quest for the repossession of Kent Island. As Claiborne recaptured his island, Richard Ingle overran St. Mary's City and overthrew Lord Baltimore's authority in Maryland in the name of Parliament and Protestantism. Recognizing the nature of Claiborne's personal vendetta, Virginia's Council of State resolved on June 6, 1645, "concerning the government of the isle of Kent," that Claiborne was "not to intermiddle with the government." The next day marked the "sudden arrival of Sir William Berkeley," who vigorously carried the war to the Indians and personally led "a Party of Horse . . . [on] a speedy March, surprised [Opechancanough] in his Quarters, and brought him Prisoner to James-Town; where, by the Governour's Command he was treated with all the Respect and Tenderness imaginable," until treacherously murdered by a guard.⁵⁰

According to a Virginia Puritan writing in 1644, the Indian attack gave Berkeley more than a chance to score a triumph that overshadowed Claiborne, for it also "did divert a great mischiefe that was growing among us by *Sir William Barclay's* courses; for divers of the most religious and honest inhabitants, were mark't out to be plundered and imprisoned for the refusall of an Oath that was imposed upon the people, in reference to the King of England . . . ; so that it is the opinion of judicious men that if the Indians had but forborne for a month longer, they had found us in such a combustion among our selves that they might with ease have cut off[f] every man."⁵¹ After his firsthand look at violent confrontations over politics

⁴⁹ Hening, *Statutes at Large*, 125, 130, 133, 168–70, 224; LNCCR, 160; and Jon Kukla, ed., "Some Acts Not in Hening's *Statutes*: The Acts of Assembly, October 1660," *VMHB*, 83 (1975): 95.

⁵⁰ "Acts, Orders and Resolutions of the General Assembly of Virginia at Sessions of March 1643–1646," *VMHB*, 23 (1915): 229, 234, 237–38; *The Court Mercurie, Number 10, From Saturday the 7 of September, to Saturday the 14th 1644* [London], McGregor Library, University of Virginia, Charlottesville; *Mercurius Aulicus* [Oxford], August 25, 1644, pp. 1136–37, Virginia Historical Society, Richmond; Robert Beverley, *The History and Present State of Virginia*, ed. Louis B. Wright (Charlottesville, 1947), 61–63; and H. R. McIlwaine, ed., *Minutes of the Council and General Court of Colonial Virginia*, (2d edn., Richmond, Va., 1979), 503.

⁵¹ Joseph Frank, ed., "News From Virginy, 1644," *VMHB*, 65 (1957): 85; Hening, *Statutes at Large*, 240–43, 277; Morton, *Colonial Virginia*, 150–53; Babette M. Levy, "Early Puritanism in the Southern and Island

and religion in England, Berkeley evidently reconsidered the policy of forced religious uniformity and, without wavering in his commitment to the Church of England, changed his strategy from coercion to inducement while simultaneously relegating the enforcement of religious uniformity to the counties and to community pressure. Thus, in 1646 and 1647 the General Assembly exempted parishioners from the payment of "any manner of tythes or duties to any unconformist" and subjected dissenting clergymen to fines of five hundred pounds of tobacco "to be disposed of by the vestry for the use of the parish." These revised laws allowed the governor to promote religious uniformity at a distance through parsimonious vestrymen and parishioners.⁵²

It would be incorrect to suppose, however, that Virginia's achievement of consociational political stability in the 1640s was the work of one man. Berkeley's statecraft was important, but so was the character of his most powerful potential enemy, Samuel Mathews, Sr. This was first evident in the Mathews-Claiborne faction's responsible reaction to bicameralism. Not wanting to be outmaneuvered by the new governor, the merchant-councilors accepted the newly created House of Burgesses as a legitimate political arena. They induced Thomas Stegg, the Virginia factor of Maurice Thompson, to resign from the Council of State and, by whatever means, win election to the new lower house and secure the speaker's chair for their faction. Moreover, by the beginning of 1645 Mathews had written authorization from the earl of Warwick, commander of Parliament's fleet and of its commission charged with oversight of colonial affairs, to bring Virginia, if Mathews chose, into Parliament's fold. Warwick gave Mathews letters urging the Virginians to "make election of [a] Governor," allowing them "liberty to chouse the present Governor if you shall see Cause," and "if not . . . recommend[ing] to your respecte Captain Mathews a man of approved affection to the good of that plantation against whom if you shall except you may use your owne freedom."⁵³

During the 1640s Parliament's fleet was engaged against the royalists, and

Colonies," *Proceedings of the American Antiquarian Society*, new ser., 70 (1960): 123–26; and "Acts, Orders and Resolutions," 237. For the importance of a clergyman's legal right to sue recalcitrant parishioners for tithes owed, see Clara Ann Bowler, "The Litigious Career of William Cotton, Minister," *VMHB*, 86 (1978): 281–94. Eventually the delegation of ecclesiastical power to the vestries brought the predominance of the laity in Virginia's colonial church that thwarted later attempts to impose an Anglican hierarchy within the colony. But Berkeley's moderate, decentralizing measures made Virginia unattractive to politically aggressive Puritans without coercing moderate nonconformists (or low-church Anglicans inclined to de facto local autonomy) and without inciting the very social discord that uniformity of religion was meant to prevent. William H. Seiler, "The Anglican Parish in Virginia," in Smith, *Seventeenth-Century America*, 119–42; and Darrett B. Rutman, "The Evolution of Religious Life in Early Virginia," *Lex et Scientia*, 14 (1978): 190–214. The best published account of Virginia's Puritans documents another instance of Berkeley's post-1645 tactics: his extension of a three-year stay of expulsion to his own Puritan-leaning chaplain, Thomas Harrison, "in the hopes that such a brilliant preacher might be brought to conform." Levy, "Early Puritanism in the Southern and Island Colonies," 127.

⁵² Hening, *Statutes at Large*, 311–12, 341–42.

⁵³ Kukla, *Speakers and Clerks of the Virginia House of Burgesses*, 10–12, 35–37; "Surrender of Virginia to the Parliamentary Commissioners, March, 1651–2," *VMHB*, 11 (1903–04): 34, 38; and Warwick to "Virginia"; Warwick to "Cap. Mathews"; Warwick to "Virginia," n.d., British Library, London, Stowe Manuscripts, Lord Warwick Correspondence, ca. 1646–48, ff. 124–25 [hereafter, Warwick Correspondence]. Internal evidence suggests that the earlier letters (f. 124) were written before Warwick learned of the April 1644 Indian attack. For other evidence of Warwick's policies, see J. H. Lefroy, comp., *Memorials of the Discovery and Early Settlement of the Bermudas, or Somers Islands, 1515–1681*, 1 (London, 1877), 586–91, 600–03. When Charles I dismissed Algernon Percy, tenth earl of Northumberland, from the office of lord high admiral on July 1, 1642,

Warwick left Virginia alone. Nevertheless, the commissioners claimed power to do "all Things which they shall from Time to Time find most fit and advantageous to the well-governing . . . and chiefly to the Preservation and Advancement of the true Protestant Religion amongst the said Planters." Both Berkeley and Mathews knew that the commissioners were ready to appoint or remove colonial governors at will "for the better governing and preserving the said Plantations and Islands from open Violence and private Disturbance and Distractions." In 1652, with the war won and Charles I beheaded, Parliament did send its fleet to Virginia. In the interim, however, neither Warwick nor his Virginia allies acted to unseat Berkeley. Warwick recommended Mathews "as a man of Integrity" but cautioned him to use his "owne discretion" and to "proceed toward a transaction with most safety and successe." He offered Mathews "the ships in harbor to be assistant" but also reminded him that the commission intended Virginia to remain "plentifull in it selfe, so honorable in the eyes of others, that it may be a flourishing Comonwealth." In short, Warwick wanted Mathews to persuade the Virginians to change their government but not to remove Berkeley at the expense of "open Violence and private Disturbance and Distractions."⁵⁴

Unlike Claiborne when he blundered toward Kent Island, Warwick and Mathews put "the peace and advancement of the Colony" first, thus allowing Berkeley to defeat the Indians, moderate his religious policies, and continue his government in Virginia. This conduct by Berkeley's political rivals was critically important. Consociational stability came to Virginia when the colony's rival leaders—one in power and one out—tacitly agreed on the legitimacy of Virginia's government. Warwick and Mathews wanted effective colonial self-government, and neither challenged Berkeley's royalist administration until 1652. Can stronger proof for the prior achievement of political stability be imagined than the carefully negotiated and entirely peaceful transfers of authority that occurred in Virginia in 1652, and again in 1660, "without damage or harm to any, or the loss of a drop of blood"?⁵⁵

WHAT ABOUT BACON'S REBELLION? Does that event confirm the orthodoxy of chaos? Or does it actually prove that lasting political stability had come to pre-Restoration Virginia? The evidence permits an emphatic answer. Bacon's Rebellion is explicable

Parliament appointed Robert Rich, second earl of Warwick, to command the fleet. Warwick, who was the major stockholder in the Bermuda Company, one of the leading investors of the defunct Virginia Company, and a participant in other joint and private ventures during the previous four decades, "had a hand," wrote J. H. Hexter, "in every important overseas enterprise, Puritan or not, legitimate or not. So long as it was a trans-Atlantic affair, Warwick was in on it." On November 2, 1643, Parliament named Warwick "Governor in Chief and High Lord Admiral of all those Islands, and other Plantations inhabited, planted or belonging to any [of] His Majesty's the King of England's Subjects," and placed him at the head of a commission of six peers and twelve MPs responsible for colonial government. The Warwick commission clearly was dominated by those whom Hexter called the "middle group" in English politics during the early years of the Civil War, including Lords Manchester, Say and Sele, Wharton, and Robartes, and Sir Arthur Haselrig, Sir Gilbert Gerard, and John Pym. Hexter, *The Reign of King Pym* (Cambridge, Mass., 1941), 85; Andrews, *Colonial Period of American History*, 1: 167; and Wesley Frank Craven, Jr., "The Life of Robert Rich, Second Earl of Warwick, to 1642" (Ph.D. dissertation, Cornell University, 1928).

⁵⁴ Warwick Correspondence, ff. 124–25.

⁵⁵ *Ibid.*; and "Surrender of Virginia to the Parliamentary Commissioners, March, 1651–2," 34.

only in the light of the basic solidity attained by Virginia's provincial institutions during twenty years of imposed or control stability followed by a fifteen-year transition to consociational stability and then by thirty years of effective government from the 1640s to the early 1670s.⁵⁶

Between 1660 and 1675 Charles II thoughtlessly jeopardized these gains. He gutted Berkeley's mercantilist program for diversified economic growth, and the tobacco economy plummeted into depression. He gave away the Northern Neck, and the Virginians paid for it. He "granted *all* of Virginia to Lords Arlington and Culpeper for thirty-one years," and the Virginians paid to buy it back. Finally, he twice humiliated Berkeley by ordering the construction of useless and expensive forts at Point Comfort, and the Virginians paid for them. As the direct result of Charles II's actions, provincial taxes soared to extraordinary per capita levels between 1671 and 1675.⁵⁷ As late as 1673 Berkeley was able to placate a group of fourteen Surry County planters protesting the colony's high taxes,⁵⁸ and yet the crown's action had seriously diminished the governor's capacity for political brokerage.⁵⁹ When frenzied fear of Indian attack swept the frontier settlements in 1675 and 1676, this fact suddenly became apparent.

Angry planters for whom any Indian was a fair target found their leader in Nathaniel Bacon, Jr., who initially sought only a legal commission to lead a volunteer army of frontier militiamen and their officers to fight Indians, any Indians.⁶⁰ Had Berkeley quietly sacrificed the colony's peaceful tributary Indian allies to the frontiersmen's wrath, he might have given Bacon a commission and let the militiamen dissipate their energies in ineffective marches through the back-country.⁶¹ Instead, Berkeley was reluctant to endorse treachery against the Indians,

⁵⁶ For a survey of three centuries of interpretation, see Jane Carson, *Bacon's Rebellion, 1676-1676* (Jamestown, Va., 1976). For a calendar of microfilmed records from British repositories, see John Davenport Neville, comp., *Bacon's Rebellion: Abstracts of Materials in the Colonial Records Project* (Jamestown, Va., [1976]). Wilcomb E. Washburn's study, although tending to favor Berkeley, remains the standard monograph about the rebellion, but it should be supplemented with Shea's dispassionate appraisal. See Washburn, *The Governor and the Rebel: A History of Bacon's Rebellion in Virginia* (Chapel Hill, 1957); and Shea, *Virginia Militia in the Seventeenth Century*, 97-121.

⁵⁷ Joan de Lourdes Leonard, "Operation Checkmate: The Birth and Death of a Virginia Blueprint for Progress, 1660-1676," *WMQ*, 3d ser., 24 (1967): 44-74. Between 1671 and 1675, by his actions in the cases of the Arlington-Culpeper grant and the fort, Charles II cost every tithable Virginian two hundred forty pounds of tobacco in extra taxes. The Point Comfort fort was meant to protect the tobacco ships of English merchants, not the colonists. At its site the James River was wider than cannon range, however, and (as Virginians could have predicted) the fort failed to prevent the loss to the Dutch of a royal frigate and twenty ships in 1667 or a second Dutch attack against the tobacco fleet in 1673. Shea, *Virginia Militia in the Seventeenth Century*, 86-96. For two years, Virginians paid sixty pounds of tobacco per poll in extra taxes to buy out Arlington and Culpeper's grant, only to face another levy of sixty pounds per poll for the effort to obtain a charter to protect the colony against the fickle royal will. CO 1/39, f. 244. Also see Billings, *Old Dominion in the Seventeenth Century*, 280. The charter had nearly passed the seals when news of Bacon's Rebellion reached London and changed the king's mind; Thomas Jefferson Wertenbaker, "The Virginia Charter of 1676," *VMHB*, 56 (1948): 263-66.

⁵⁸ Billings, *Old Dominion in the Seventeenth Century*, 243-44, 258-67; and Morgan, *American Slavery, American Freedom*, 246-49.

⁵⁹ In Massachusetts, according to T. H. Breen, "ballooning levies strained relations between different levels of authority, especially local and colonial, and as time passed, the position of established political brokers in this society was seriously eroded." For a suggestive parallel between Massachusetts and Virginia, see Breen, "War, Taxes, and Political Brokers: The Ordeal of Massachusetts Bay, 1676-1692," in his *Puritans and Adventurers: Change and Persistence in Early America* (New York, 1980), 81-105, esp. 82.

⁶⁰ Shea, *Virginia Militia in the Seventeenth Century*, 103-08; and Washburn, *Governor and Rebel*, 36-37, 40-48.

⁶¹ Morgan, *American Slavery, American Freedom*, 260-61; and Shea, *Virginia Militia in the Seventeenth Century*, 105-08, 110-12.

and Bacon marched anyway. Finally, in May and June, Berkeley made a series of tactical mistakes that forced Bacon into open rebellion and brought Virginia several months of avoidable violence.⁶² An Indian campaign became a civil war that neither side wanted.

The peak of Bacon's popularity came in August, when Berkeley shocked many prominent planters by fleeing across Chesapeake Bay to Virginia's Eastern Shore when a party of Indians roamed the peninsula between the York and Rappahannock rivers.⁶³ But Bacon "badly overplayed his hand" when he burned Jamestown and began, early in October, accepting servants and slaves into his army. Bacon's recruitment of servants and slaves made "all masters his Enemies," and his support promptly withered among the militiamen who had followed him to fight Indians.⁶⁴ Bacon died on October 26, and within a few weeks the entire rebellion collapsed.

Berkeley dealt unmercifully with Bacon's principal associates, squabbled with both the king's investigating commissioners and Lieutenant Governor Herbert Jeffreys, and eventually was recalled to England where he died in 1677.⁶⁵ It is noteworthy that Bacon's Rebellion had scarcely any lasting impact on Virginia. The rebels neither sought institutional change nor produced a revolutionary program.⁶⁶ Historians have never been able to agree about what relationship, if any, the rebellion had to social change in the second half of the seventeenth century.⁶⁷ The rebellion left few lingering animosities, and early in the 1680s Berkeley's old loyalists and Bacon's old followers closed ranks to oppose Stuart colonial policies in Virginia.⁶⁸ The crown's new governors—Thomas Culpeper, Francis Howard, and Sir Edmund Andros—did not identify themselves with the colony as had Wyatt, Berkeley, and Richard Bennett and were frequently opposed by Virginia's councilors, burgesses, county leaders, and populace. The big change after 1680 was not in

⁶² Shea, *Virginia Militia in the Seventeenth Century*, 100–01, 108–09, 112.

⁶³ *Ibid.*, 108–09.

⁶⁴ *Ibid.*, 114.

⁶⁵ Washburn, *Governor and Rebel*, 92–151.

⁶⁶ *Ibid.*, 49–67; and Morgan, *American Slavery, American Freedom*, 262–63.

⁶⁷ Thad W. Tate, "The Seventeenth Century Chesapeake and Its Modern Historians," in Tate and Ammerman, *The Chesapeake in the Seventeenth Century*, 19–24; Thomas Jefferson Wertenbaker, *Virginia Under the Stuarts* (Princeton, 1914), 115–45, *Torchbearer of the Revolution: The Story of Bacon's Rebellion and Its Leader* (Princeton, 1940); Warren M. Billings, "The Causes of Bacon's Rebellion: Some Suggestions," *VMHB*, 78 (1970): 409–35; Menard, "Tobacco Industry in the Chesapeake Colonies," 136–37; T. H. Breen, "A Changing Labor Force and Race Relations in Virginia, 1660–1710," *Journal of Social History*, 7 (1973–74): 3–25; John C. Rainbolt, *From Prescription to Persuasion: Manipulation of Seventeenth-Century Virginia Economy* (Port Washington, N.Y., 1974), 92–98; Wesley Frank Craven, *White, Red, and Black: The Seventeenth-Century Virginian* (Charlottesville, Va., 1971); Edmund S. Morgan, "Head Rights and Head Counts: A Review Article," *VMHB*, 80 (1972): 361–71; Wilcomb E. Washburn, "Virginia's First Families and the First Families of Virginia," in Roscoe D. Hughes and Henry Leidheiser, Jr., eds., *Exploring Virginia's Human Resources* (Charlottesville, Va., 1965), 8–18; and Bailyn, "Politics and Social Structure in Virginia." As his *Governor and Rebel* was going to press, Washburn "was able to add a footnote," recording his "disagreement with [Bailyn's] thesis" about long-term social causes of the rebellion. Washburn to the author, December 12, 1979. Washburn's note described Bailyn's essay as "a perceptive analysis of the class antagonisms existing at the time" but concluded: "That these oppositions existed I have pointed out. That they express 'deeper elements' than the more 'immediate causes' of 'race relations and settlement policy,' as Bailyn believes, I deny." See Washburn, *Governor and Rebel*, 235 n. 3; and Bailyn, "Politics and Social Structure in Virginia," 102.

⁶⁸ Rainbolt, "Alteration in the Relationship Between Leadership and Constituents in Virginia," 422–23; Jane D. Carson, "Lady Frances Berkeley," in Edward T. James et al., eds., *Notable American Women, 1607–1950*, 1 (Cambridge, Mass., 1971), 135–36; and Kukla, "Robert Beverley Assailed," 421–23, *Speakers and Clerks of the Virginia House of Burgesses*, 65–92.

Virginia's internal stability but in its relationship to the London government.⁶⁹

PRE-RESTORATION VIRGINIA MEETS ALL SEVEN of the criteria suggested by Greene as evidence for political stability.⁷⁰ First, until 1676 Virginia experienced no episodes of internal collective violence or civil disorder. Second, longstanding issues that might have polarized the colony were regularly resolved through political action: examples include the carefully legal removal of Governor Harvey, the confirmation of land titles, Berkeley's outflanking of the would-be tobacco monopolists, the enforcement of religious uniformity after 1646, and the accommodating ecclesiastical arrangements negotiated on the colony's submission to Parliament in 1652. Third, the Virginians' routine acceptance of their institutional and leadership structures was evident as early as the 1620s and repeatedly affirmed both by petitions for royal recognition of the General Assembly and by regular elections of burgesses. This acceptance of existing institutions was once again confirmed by the constitutional deliberations of the General Assembly in April and May 1652 after the submission to Parliament. Fourth, a regularization of relations among the several branches and levels of government—governor, council, assembly, and counties—occurred between 1623 and 1643. Fifth, in 1624, 1652, and 1660 major transfers of legitimate authority occurred in potentially explosive circumstances without disruption of the polity. In Virginia, the orderly and constitutional succession of one governor by another governor was routine practice, even when Governor Harvey was driven from office in 1635. Sixth, in many ways at various times (and especially through the responsible political leadership of Mathews and Berkeley in the 1640s) factional activity was channeled into routine and functional political avenues.

The evidence concerning Greene's remaining criterion—low levels of turnover among leaders—also supports the case for political stability in pre-Restoration Virginia. Although the destruction of Virginia records limits the data available for comparison to the four counties of Accomack/Norhampton, Charles City, Lower Norfolk, and York between 1639 and 1660, the continuity of burgesses representing these four counties in the General Assembly is reflected in the fact that 59.2 percent of the members of any given assembly had previous experience as burgesses.⁷¹ A century later (when Virginia was a model of stability) between 1728

⁶⁹ John C. Rainbolt, "A New Look At Stuart 'Tyranny': The Crown's Attack on the Virginia Assembly, 1676–1689," *VMHB*, 75 (1967): 387–406; and Stephen Saunders Webb, "'Brave Men and Servants to his Royal Highness': The Household of James Stuart in the Evolution of English Imperialism," *Perspectives in American History*, 8 (1974): 55–80, *The Governors-General: The English Army and the Definition of the Empire, 1569–1681* (Chapel Hill, 1979), 378–435.

⁷⁰ Greene, "The Growth of Political Stability," 32–33 n. 17.

⁷¹ In Accomack/Norhampton, Charles City, Lower Norfolk, and York counties between 1639 and 1660, the average percentage of experienced burgesses in the General Assembly was 59.2 percent. Although sufficient evidence for Charles City and York counties survives to allow the compilation of this statistic, the available data from these counties are not as complete as for the two counties of Accomack/Norhampton and Lower Norfolk, in which between 1639 and 1660 the average percentage of experienced burgesses in the General Assembly was 68.8 percent. For lists of all known members of the Virginia General Assembly before 1660, see Cynthia Miller Leonard, comp., *The General Assembly of Virginia, July 30, 1619–January 11, 1978: A Bicentennial Register of Members* (Richmond, 1978), 16–36. The survival of Eastern Shore and Lower Norfolk County records for these decades makes this evidence the most nearly complete for the period. Recurrent hiatuses in the data for other counties preclude the compilation of comparable statistics, and names of assembly members in the 1620s and 1630s frequently are unknown.

and 1761 the comparable figure for these same counties was 57.6 percent.⁷² For the English House of Commons in 1604 the equivalent percentage of experienced members of Parliament was only 55 percent.⁷³ For the House of Commons between 1754 and 1790 Namier and John Brooke computed the average percentage of experienced MPs at 76.6 percent.⁷⁴ Finally, for the legislature of Pennsylvania between 1727 and 1755, Alan Tully's data show that the average percentage of experienced members was 85.6 percent.⁷⁵ During the second quarter of the seventeenth century, according to these seven criteria for stability, colonial Virginians created a place where public transactions happened, and could be expected to happen, within regular patterns and without illegal violence.

Colonial Virginia's history throws into high relief some neglected origins of our political heritage, for the Virginia colonists brought to their confrontation with the American wilderness the jumbled uncertainties and confusions that plagued seventeenth-century Europeans. Some of their leaders were driven by an Augustinian piety, but most possessed an empirical temperament. They remind us of Aristotle's credo for all working politicians: "the best is often unattainable, and therefore the true lawmaker or statesman ought to be acquainted not only with that which is best in the abstract but also with that which is best considering the circumstances."⁷⁶ The early Virginians' lasting achievement was a stable indigenous political order and the beginning of a political culture embodied more in institutions and practices than in rhetoric and theory. Transmitted to us through their late eighteenth-century descendants, their contribution to American statecraft is a reminder of an underlying Aristotelian temperament in American political institutions and practices. This tradition was two centuries old when

⁷² *Ibid.*, 74–90. For this period the membership lists for these four counties are complete.

⁷³ J. H. Hexter, "The Birth of Modern Freedom," *Times Literary Supplement*, January 21, 1983, p. 53. In his review of P. W. Hasler's *The History of Parliament: The House of Commons, 1558–1603* (London, 1980), Hexter wrote: "Level of experience in a deliberative body can have a significant effect on its ability to deal with the business before it. In an Elizabethan House of Commons, however, the experience of present members is *not* adequately measured by counting how many had been in the *previous* parliament. On a very hasty runthrough of the M[embers] in the[se] register[s] of members, it turned out that one-sixth had sat in a previous parliament but not in the previous one. A more careful canvass of the members elected in 1604 indicates that fully 55 per cent had previous parliamentary experience rather than the 40 per cent that the figure given in Appendix X suggests. We are again faced by a set of statistics of small use for significantly measuring what it is supposed to measure." Jack P. Greene's reliance on the less revealing statistic also precludes meaningful comparisons using his figures. See Greene, "Legislative Turnover in British America, 1696 to 1775: A Quantitative Analysis," *WMQ*, 3d ser., 38 (1981): 442–63.

⁷⁴ For the statistics on which my tabulations are based, see Sir Lewis Namier and John Brooke, *The History of Parliament: House of Commons, 1754–1790*, 1 (New York, 1964), 98. Jack Greene erred in extending to Namier and Brooke (whose definitions are clear) the reprimand deserved by Romney Sedgwick for his failure "to define precisely what [was] meant by 'new member'"; "Legislative Turnover in British America," 445 n. 9. See Sedgwick, *The History of Parliament: House of Commons, 1715–1754*, 1 (New York, 1970), 155. Sedgwick's imprecision prevents meaningful comparisons using his figures.

⁷⁵ For the statistics on which my tabulations are based, see Alan Tully, *William Penn's Legacy: Politics and Social Structure in Provincial Pennsylvania, 1726–1755* (Baltimore, 1977), 181–82.

⁷⁶ Aristotle, *Politics*, bk. 4, chap. 1. "Aristotle, it might be said, was better at describing statesmen than saints"; Fiering, *Moral Philosophy at Seventeenth-Century Harvard*, 17. Peter Laslett wrote that "empirical medicine, rather than philosophy, seems to be the model for the man who sets out to comment on political matters. Locke the doctor, rather than Locke the epistemologist, is the man we should have in mind when we read his work on *Government*." Compare Laslett's observation with Alfred North Whitehead's comments on Aristotle, "the son of a doctor." Laslett, ed., *Two Treatises on Government* (Cambridge, 1960), 98–99; and Whitehead, *Science in the Modern World* (New York, 1927), 43. Also see David Newsome, *Two Classes of Men: Platonism and English Romantic Thought* (New York, 1974), 1–7; Isaiah Berlin, *The Hedgehog and the Fox* (London, 1953), reprinted in Henry Hardy and Aileen Kelly, eds., *Russian Thinkers* (New York, 1978), 22–81.

Thomas Jefferson defined his own version of Aristotle's outlook in a letter of 1803 to John Randolph of Roanoke. "I see too many proofs of the imperfection of human reason to entertain wonder or intolerance at any difference of opinion on any subject; . . . experience having taught me the reasonableness of mutual sacrifices of opinion among those who are to act together, for any common object, and the expediency of doing what good we can; when we cannot do all we would wish."⁷⁷ The Virginians' initial confusion about effective political authority was only one facet of a common dilemma in the postmedieval West about the nature of ultimate authority in the cosmos.⁷⁸ Perry Miller put it well in the final sentence of his essay on Virginia. "In a world where the ancient landmarks were fading, where the will of God was becoming ambiguous to man's reading, the one remaining certainty, the one institution which could plead at least the excuse of utility, was the organized rights of Englishmen, exercised and protected in an elective assembly."⁷⁹ The Old Dominion's contribution to American life was not a Platonic vision of eternal Truth but an Aristotelian grasp of human politics.

⁷⁷ Jefferson to Randolph, December 1, 1803, Library of Congress, Washington, D.C., Thomas Jefferson Papers, ser. 1.

⁷⁸ Theodore K. Rabb, *The Struggle for Stability in Early Modern Europe* (New York, 1975).

⁷⁹ Perry Miller, "Religion and Society in the Early Literature: The Religious Impulse in the Founding of Virginia," *WMQ*, 3d ser., 6 (1949): 41.

The Hypothesis of Middle-Class Formation in Nineteenth-Century America: A Critique and Some Proposals

STUART M. BLUMIN

IN 1976 BURTON BLEDSTEIN PUBLISHED a book in which he purported to find within an emerging nineteenth-century "culture of professionalism" nothing less than "a cultural process by which the middle class in America matured and defined itself." "The middle class in America," Bledstein wrote, "appeared as a new class with an unprecedented enthusiasm for its own forms of self-expression, peculiar ideas, and devices for self-discipline."¹ This proposition was something rather new in American historical literature, for it identified not a "middle-class nation," or a vaguely ascendant "middle-class culture," but a distinctively new, definable, even self-conscious middle class, a coherent social formation that from the middle of the nineteenth century set itself off in specific ways from the rest of American society. Two years later, in his book on religious revivals and social change in Rochester, Paul E. Johnson stated the case for middle-class formation even more boldly. To Johnson, the efforts of "whig politicians, industrial moralizers, temperance advocates, missionaries, and family reformers" to "build a world that replaced force, barbarism, and unrestrained passion with Christian self-control" constituted "the moral imperative around which the northern middle class became a class."² Also in 1978, Paul Boyer wrote, somewhat more temperately, that these efforts at moral and social reform helped "an embryonic urban middle class define itself," and he described a century-long struggle by the middle class to "achieve a greater degree of internal order and cohesion."³ And in 1981, Mary P. Ryan framed her stimulating analysis of family life, evangelical reform, and urban-industrial development in Utica, New York, as "a chronicle of the formation of a new American middle class," complaining as she did so that the middle class "is largely a residual category in American historiography, the assumed, but largely unexamined, context for much of the writing about popular culture and reform movements. . . . Historians

¹ Bledstein, *The Culture of Professionalism: The Middle Class and the Development of Higher Education in America* (New York, 1976), ix.

² Johnson, *A Shopkeeper's Millennium: Society and Revivals in Rochester, New York, 1815-1837* (New York, 1978), 8.

³ Boyer, *Urban Masses and Moral Order in America: 1820-1920* (Cambridge, Mass., 1978), 61, 179.

have hardly begun to analyze middle Americans as a class unto themselves.”⁴

This sudden urge to analyze middling folk as “a class unto themselves” no doubt owes much to the efforts of other recent historians to define working classes—and, to a lesser extent, urban upper classes—as distinct social formations of the nineteenth century. Inspired by E. P. Thompson’s insistence that class is a concrete, human, historically specific phenomenon, the “new working-class historians” have shown not that working men and women constituted a single and self-conscious working class but that many of them inhabited a social, moral, and ideological world in many ways different from and antagonistic to that of the professionals and entrepreneurs (and particularly the nonartisanal entrepreneurs) who came to be called the middle class.⁵ At the other extreme, a smaller number of historians have demonstrated how very wealthy city dwellers strove to establish themselves as an upper class, superior in manners to, and socially and institutionally insulated from, less successful members of the bourgeoisie.⁶ These often-impressive historical studies of specific emerging classes at the top and bottom of nineteenth-century society have emphasized both the lines of divergence of rich and poor from intermediate social strata and the general relevance of the concept of class formation to the nineteenth century. In any case, middle-class formation has now been offered, clearly and forcefully, as a central hypothesis for the study of the evolution of nineteenth-century American society. Indeed, there are even signs that the idea of middle-class formation is already passing from hypothesis to assumption. In his recent study of antebellum New York City, for example, Edward K. Spann wrote freely and without explanation of New York’s “emerging middle class,” as though the emergence of this class was a well-demonstrated and widely understood social fact.⁷ And in a still more recent study, Karen Halttunen wrote in

⁴ Ryan, *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865* (Cambridge, 1981), xiii, 13. Also see Sean Wilentz, “Artisan Origins of the American Working Class,” *International Labor and Working Class History*, no. 19 (1981): 4; “Artisan Republican Festivals and the Rise of Class Conflict in New York City, 1788–1837,” in Michael H. Frisch and Daniel J. Walkowitz, eds., *Working-Class America: Essays on Labor, Community, and American Society* (Urbana, Ill., 1983), esp. 62; and John Shanklin Gilkeson, Jr., “A City of Joiners: Voluntary Associations and the Formation of the Middle Class in Providence, 1830–1920” (Ph.D. dissertation, Brown University, 1981).

⁵ See Thompson, *The Making of the English Working Class* (London, 1963); “Eighteenth-Century English Society: Class Struggle Without Class?” *Social History*, 3 (1978): 133–65; Herbert G. Gutman, *Work, Culture, and Society in Industrializing America: Essays in American Working-Class and Social History* (New York, 1976); Sean Wilentz, *Chants Democratic: New York City and the Rise of the American Working Class, 1788–1850* (New York, 1984); Bruce Laurie, *Working People of Philadelphia, 1800–1850* (Philadelphia, 1980); Paul G. Faler, *Mechanics and Manufacturers in the Early Industrial Revolution: Lynn, Massachusetts, 1780–1870* (Albany, N.Y., 1981); Daniel J. Walkowitz, *Worker City, Company Town: Iron and Cotton Worker Protest in Troy and Cohoes, New York, 1855–1884* (Urbana, Ill., 1978); John T. Cumbler, *Working-Class Community in Industrial America: Work, Leisure, and Struggle in Two Industrial Cities, 1880–1930* (Westport, Conn., 1979); Susan E. Hirsch, *Roots of the American Working Class: The Industrialization of Crafts in Newark, 1800–1860* (Philadelphia, 1978); and Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976). Many of the essays in Frisch and Walkowitz’s *Working-Class America* are relevant. Also see Milton Cantor, ed., *American Working-Class Culture: Explorations in American Labor and Social History* (Westport, Conn., 1979).

⁶ See, especially, Edward Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973); Frederic Cople Jaher, *The Urban Establishment: Upper Strata in Boston, New York, Charleston, Chicago, and Los Angeles* (Urbana, Ill., 1982); Ronald Story, *The Forging of an Aristocracy: Harvard and the Boston Upper Class, 1800–1870* (Middletown, Conn., 1980). For three historical works by the sociologist E. Digby Baltzell, see *Philadelphia Gentlemen: The Making of a National Upper Class* (Glencoe, Ill., 1958), *The Protestant Establishment: Aristocracy and Caste in America* (New York, 1964), and *Puritan Boston and Quaker Philadelphia: Two Protestant Ethics and the Spirit of Class Authority and Leadership* (New York, 1977).

⁷ Spann, *The New Metropolis: New York City, 1840–1857* (New York, 1981), 243.

an equally offhand manner that antebellum sentimentalism was “central to the self-conscious self-definition of middle-class culture during the most critical period of its development.”⁸ Has it really been established that this was the “most critical period” of middle-class formation or even that middle-class formation is a useful concept? Ryan’s summons for historical analysis of “middle Americans as a class unto themselves” is as badly answered by those who uncritically accept the idea as it is by those who ignore it entirely.

It is interesting to note in this connection that even the works I cite, which instigated this new departure in American social history, focus partly or even mainly on other issues besides those of middle-class formation. Although Bledstein’s book begins with a long discourse on “The Advantage of Being Middle Class,” it ultimately turns toward the analysis and critique of a “culture of professionalism” that can be applied only with difficulty to more than a small segment of the nineteenth-century middle class as it is usually defined or imagined. More important, Bledstein himself seemed to recognize this difficulty and focused increasingly not on the emergence or maturation of a new social class but on the costs to society of an exclusive and deceitful mystique of professionalism.⁹ Johnson’s remarks about the making of the Rochester middle class are—surprisingly, in view of their boldness—confined to the introductory pages of his book, whereas Boyer’s statements about middle-class self-definition and cohesion express only a peripheral theme within his rich narrative of urban moral reform. Of these authors, only Ryan kept the argument for middle-class formation consistently in view—the evolving family that is the substantive focus of the book is, to use her apt metaphor, the “cradle” of the middle class. But she did not always give it her unqualified support. At times she stressed the defensive rather than the expansive aspects of the new strategies of child rearing and enhancement of family status around which her middle class cohered, and at times she hesitated to make too large a claim for the period she examined, instead pushing the process of class formation safely ahead into following generations. “The making of the middle class in the industrial age was *conditioned by* family changes dating from the canal era.”¹⁰ None of this reduces Ryan’s commitment to the central concept, but one cannot help noticing that even here we have a somewhat qualified and not quite complete “chronicle of the formation of a new American middle class.”

WHAT, THEN, SHALL WE MAKE OF MIDDLE-CLASS FORMATION as a hypothesis for the examination of nineteenth-century American social development? Is it an essential idea, either in its own right or as a central component of a larger hypothesis concerning the development of classes in modern society? If so, precisely how should the hypothesis of middle-class formation best be stated? Is it, on the other hand, a poor idea, one that obscures and distorts more than it reveals? Should it simply be dismissed? That it should be dismissed is suggested by at least three major

⁸ Halttunen, *Confidence Men and Painted Women: A Study of Middle-Class Culture in America, 1830–1870* (New Haven, 1982), xvii. Also see *ibid.*, 194, 197.

⁹ Bledstein, *Culture of Professionalism*, esp. 334.

¹⁰ Ryan, *Cradle of the Middle Class*, 239 [My emphasis]. Also see, for example, *ibid.*, 155.

schools of historical thought in America, each of which challenges the concept of middle-class formation in a different way. To historians of the "consensus school," for example, the term "middle class" refers to the bourgeois, entrepreneurial, privatistic values that pervade all levels of society—in other words, to a national culture of liberalism—and does not mean a distinct social formation within American society. In the United States, according to this view, the culture that in Europe was associated with a particular class (located hierarchically between a formally aristocratic upper class and a decidedly plebeian lower class) broke out of its class boundaries to provide a common system of belief and action and a powerful source of national cohesion. Hence, middle-class formation is best seen not as a type or aspect of class formation but as the development of bourgeois liberalism within the culture as a whole. This is why Louis Hartz could note, in *The Liberal Tradition in America*, that "the Americans, a kind of national embodiment of the concept of the bourgeoisie, have . . . rarely used that concept in their social thought," for "a triumphant middle class . . . can take itself for granted."¹¹ To do otherwise, to posit and examine the middle class as a specific and distinct social group within a hierarchically organized society of classes, is, Hartz implied, to misrepresent fundamentally the character of American society, as well as the middle class triumph that has built a national consensus rather than a class.

A second challenge to the hypothesis of middle-class formation comes from those historians who recognize the existence in America of deep and abiding conflicts but who associate these conflicts, especially as they manifest themselves in politics, with ethnic and religious divergence rather than with class. In an essay intended in part "to kill economic determinism dead so that it stays dead in American historiography," Lee Benson specified eight conditions under which social class cohesion and interclass conflict would be reduced: a widespread and longstanding equality of legal status, a wide range of incomes and a large number of income strata, a high rate of perceived social mobility, a high rate of geographic mobility, high rates of foreign immigration from antagonistic cultures and of internal migration between areas with antagonistic cultural patterns, highly developed communications among and between diverse ethnocultural groups, a small number of lingering extralegal inequalities, and a high value placed on individual competition within the wealth-based stratification system.¹² The fifth and sixth of these conditions do not suggest an overall reduction of group cohesion and intergroup conflict (as the "consensus" historians would have it); rather, they suggest a channeling of that cohesion and conflict along ethnocultural lines. And because all eight conditions prevailed in nineteenth-century America, Benson concluded that "ethnocultural and religious attributes, not economic attributes, mainly functioned as the concrete bases of group cohesion and social and ideological conflict." Benson and others have substantiated this proposition with numerous studies of political ideology and

¹¹ Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought Since the Revolution* (New York, 1955), 51–52.

¹² Benson, "Group Cohesion and Social and Ideological Conflict: A Critique of Some Marxian and Tocquevillian Theories," *American Behavioral Scientist*, 16 (1973): 741–67, esp. 757, 748.

partisan alignment that either deny the existence of a politically relevant middle class or portray a middle class divided against itself on ethnocultural issues.¹³

A third challenge comes from various Marxist historians and theoreticians who, while affirming the class basis of social cohesion and conflict, deny the significance of intermediate classes as social groups, save for the temporarily intermediate (and ultimately dominant) class that represents a new and ascending mode of production. Other intermediate groups are both temporary and illusory, consisting of mere appendages of the dominant class or of transitional classes that represent the remains of former modes of production.¹⁴ Thus, while Marxists recognize (as did Marx himself) the existence of intermediate groups in any given society at any given time,¹⁵ many also insist that the concept of "middle class" obscures the fundamental two-class structure of capitalist (or feudal) society, and distorts the concept of class by creating an artificial social group that bears no definable and essential relation to the means of production and contains within it no potential for genuine coherence and consciousness. Even the new and apparently robust intermediate formations of mature capitalist society are dismissed in this way. Indeed, several contemporary Marxists, eager to demonstrate that the "new middle class" does not threaten Marxism as a general theory of historical development, have devoted considerable attention to theoretical demonstrations of the essential differences between intermediate groups and "the two basic classes of a capitalist formation," or the "contradictory class locations" (and hence the classlessness) of intermediate strata, or, most recently, the idea that "middle-class formation is an expression of the class polarization process" and that the middle class, far from being "the harbinger of a new order," is "gradually disintegrating as a class."¹⁶ In sum, "There is no fixed place for the middle class, no determined role, no necessary direction or certainty of outcome when the class asserts itself. The historical existence, the place in the social division of labor, the class situation of the middle are expressions of a larger set of class relations."¹⁷

¹³ See Benson, *The Concept of Jacksonian Democracy: New York As a Test Case* (Princeton, 1961); Robert Kelley, *The Cultural Pattern in American Politics: The First Century* (New York, 1979); Ronald P. Formisano, *The Birth of Mass Political Parties: Michigan, 1827-1861* (Princeton, 1971); Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (New York, 1970), *The Third Electoral System, 1853-1892: Parties, Voters, and Political Cultures* (Chapel Hill, 1979); Michael F. Holt, *Forging a Majority: The Formation of the Republican Party in Pittsburgh, 1848-1860* (New Haven, 1969); Richard J. Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888-1896* (Chicago, 1971); and Samuel T. McSeveney, *The Politics of Depression: Voting Behavior in the Northeast, 1893-1896* (New York, 1972). In two important studies, Daniel Walker Howe and Eric Foner have associated the political culture of Whigs and mid-nineteenth-century Republicans with the "middle class," but neither has claimed that Whig or Republican conflict with Democrats or other parties was structured along class lines. See Howe, *The Political Culture of the American Whigs* (Chicago, 1979); and Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970). For a recent but unsuccessful attempt to associate political conflict with classes, see Edward Pessen, "Social Structure and Politics in American History," *AHR*, 87 (1982): 1290-1325.

¹⁴ For an excellent and relatively recent discussion of this aspect of Marxist theory, see Anthony Giddens, *The Class Structure of the Advanced Societies* (New York, 1975), 23-40.

¹⁵ See, for example, Marx's discussion of the politics of the French "petty bourgeoisie" in *The Eighteenth Brumaire of Louis Napoleon*, in Karl Marx and Friedrich Engels, *Collected Works*, 11 (New York, 1979): 103-97.

¹⁶ Nicos Poulantzas, *Classes in Contemporary Capitalism* (London, 1975), 205, 287; Erik Olin Wright, *Class, Crisis, and the State* (London, 1978), 61-87; and Dale L. Johnson, *Class and Social Development: A New Theory of the Middle Class* (Beverly Hills, Calif., 1982), 24, 106.

¹⁷ Johnson, *Class and Social Development*, 10.

Each of these objections, though substantial, can be answered and at least partly overturned. Surely the numerous recent studies of working-class culture, social life, and protest have done much to define the lower limit of the bourgeois consensus and to place a substantial number of working-class men and women beyond that limit. To be sure, some of these studies have identified groups or types of workers whose ambitions, life styles, and political values were resolutely "middle class" (Paul Faler's "loyalists" and Bruce Laurie's "revivalists" come most readily to mind), but each of these studies, and others, point to workers—"traditionalists," "radicals," or simply unlabeled toilers and strikers—who were and knew that they were clearly of another social order.¹⁸ At the same time, Edward Pessen, Frederic Cople Jaher, and Ronald Story have delineated urban upper classes that formed not merely a *haute bourgeoisie* of extremely wealthy merchants, bankers, manufacturers, and speculators but also an aspiring aristocracy, at least in the metaphoric sense of an ascriptive upper class that succeeded in varying degrees and for varying lengths of time in denying its bourgeois origins.¹⁹ Underscoring these studies of distinctive working-class and patrician cultures are several recent analyses of the dimensions of economic inequality in nineteenth-century America, all of which point to very striking increases in inequality in the antebellum era and to the maintenance of very high levels of inequality throughout the nineteenth century.²⁰ It is becoming increasingly clear, in short, that Americans diverged widely in their economic circumstances and that they translated their economic differences into significant differences in life style, outlook, and aspiration. However broad the bourgeois consensus may have been in comparison to European societies, it was not so broad that it precluded the formation of distinct classes within American society. Even the supposedly all-encompassing American bourgeoisie may well have been a class after all—the power of its values serving to reinforce rather than to destroy social class boundaries.²¹

The political historians' objection that ethnocultural groups, not classes, form the basic positive and negative reference groups of American politics may be answered

¹⁸ See Faler, "Cultural Aspects of the Industrial Revolution: Lynn, Massachusetts, Shoemakers and Industrial Morality, 1826–1860," *Labor History*, 15 (1974): 367–94; and Laurie, *Working People of Philadelphia*. Wilentz has noted the complete bifurcation by 1836 (between master and journeymen artisans) of the once-unified artisan republican ideology; "Artisan Republican Festivals," 61.

¹⁹ See Pessen, *Riches, Class, and Power before the Civil War*; Jaher, *Urban Establishment*; and Story, *Forging of an Aristocracy*, and "Class and Culture in Boston: The Atheneum, 1807–1860," *American Quarterly*, 27 (1975): 178–99.

²⁰ See Lee Soltow, *Men and Wealth in the United States: 1850–1870* (New Haven, 1975), "Economic Inequality in the United States in the Period from 1790 to 1860," *Journal of Economic History*, 31 (1971): 822–39, "The Wealth, Income, and Social Class of Men in Large Northern Cities of the United States in 1860," in James D. Smith, ed., *The Personal Distribution of Income and Wealth* (New York, 1975), 233–76; Pessen, *Riches, Class, and Power before the Civil War*; Stuart Blumin, "Mobility and Change in Ante-Bellum Philadelphia," in Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, 1969), esp. 204–05; Michael B. Katz, *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975), 44–93; Robert E. Gallman, "Trends in the Size Distribution of Wealth in the Nineteenth Century: Some Speculations," in Lee Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York, 1969), 1–25; and Craig Buettinger, "Economic Inequality in Early Chicago, 1849–1850," *Journal of Social History*, 11 (1978): 413–18. For the most recent and comprehensive account, see Jeffrey G. Williamson and Peter H. Lindert, *American Inequality: A Macroeconomic History* (New York, 1980).

²¹ It should be noted that the terms "bourgeoisie" and "middle class" are by no means interchangeable. For two valuable essays, see Peter Stearns, "The Middle Class: Toward a Precise Definition," *Comparative Studies in Society and History*, 21 (1979): 377–96; and Arno J. Mayer, "The Lower Middle Class as Historical Problem," *Journal of Modern History*, 47 (1975): 409–36.

in two ways. First, it should be noted that particular group identities can exist without expressing themselves through politics, either because of the exclusion of those groups from the political community (or merely from an issue-defining elite) or because of crosscutting identities or ideologies that, for one reason or another, gain precedence within the political arena. The latter phenomenon seems to have been particularly important in excluding class issues from American politics during certain eras when both class and ethnic feelings were intense. In her study of workingmen in antebellum Newark, for example, Susan Hirsch found both a developing working-class consciousness—expressed mainly through craft unionism—and rising ethnic antagonisms that for workers were closely connected with economic issues and preempted the position that those economic issues might have occupied in the political system. “Beset by the counterclaims of class and ethnic groups,” wrote Hirsch, “journeymen confined their struggles for power to craft unionism and ethnic politics.”²² What is important here is the simultaneous manifestation of class and ethnic identities, the latter in politics, the former in the separate, but no less revealing, institutional world of craft unionism. Thus, ethnocultural political historians may be right about the issues and identities informing American politics but would be wrong to amplify their conclusions about political behavior into general propositions about group identity in American society.

But can the same answer to the ethnocultural objection be offered on behalf of the middle class? Here a somewhat different response is required, one that stresses not actively competing ideologies but an essentially passive or even negative class “ideology,” which requires little in the way of class-based political solidarity (primarily because the political system is already structured to meet its demands) and seeks to avoid overt displays of explicit class cooperation that too obviously contradict the individualism that lies at the heart of the middle-class value system.²³ Political movements based explicitly on the grievances or aspirations of intermediate social classes are indeed rare in American history.²⁴ Is this because middle-class consciousness does not exist or because it is built around values that preclude its manifestation in politics? The latter argument, we must admit, brings us face to face with what is at least a central paradox in the concept of middle-class formation—the building of a class that binds itself together as a social group in part through the common embrace of an ideology of social atomism! Only if this is indeed merely a paradox, and not an outright contradiction, does it help explain how the middle class could have come to exist without manifesting itself in politics. This is an important issue, to which I will return shortly.

²² Hirsch, *Roots of the American Working Class*, 116. David Montgomery made essentially the same argument, as did Ira Katznelson. See Montgomery, “The Shuttle and the Cross: Weavers and Artisans in the Kensington Riots of 1844,” *Journal of Social History*, 5 (1972): 411–46; and Katznelson, *City Trenches: Urban Politics and the Patterning of Class in the United States* (Chicago, 1981).

²³ See Gerard DeGre, “Ideology and Class Consciousness in the Middle Class,” *Social Forces*, 29 (1950–51): 173–79.

²⁴ One interesting exception is the early nineteenth-century “middling interest” in Boston. See Robert A. McCaughey, “From Town to City: Boston in the 1820s,” *Political Science Quarterly*, 88 (1973): 191–213; and Andrew R. L. Cayton, “The Fragmentation of ‘A Great Family’: The Panic of 1819 and the Rise of a Middling Interest in Boston, 1818–1822,” *Journal of the Early Republic*, 2 (1982): 143–67.

Responding to the Marxists is at once complex and simple: complex because the response must involve the whole elaborate edifice of Marxist theory, simple because it can attack that edifice at a single point. The Marxist objection to the concept of middle-class formation rests ultimately on the distinction between the essential classes—classes that are generated by the capitalist mode of production—and the inessential intermediate groups that bear no significant or consistent relation to the means of production. This distinction between essential and inessential classes is derived not from observed differences in social, cultural, or ideological coherence at any given time but from the predictive aspects of what is, at bottom, a theory of past and future capitalist development.²⁵ Marxists predict—on the basis of the logical structure of this theory—that intermediate classes will simply dissolve and be absorbed into the two essential classes as capitalism becomes more fully developed. In the meantime, having no essential role to play (at best the middle class “mediates the capital/labor social relation”), intermediate classes may be safely dismissed, and the basic two-class model confirmed, even with respect to earlier stages of capitalist development. Marxists do recognize the deductive character of this argument and sometimes employ the distinction between “class” and “stratification” to give voice to the superiority of theory over mere observation. “Class is an analytic category with which the social structure is defined. Stratification describes divisions within the class structure.”²⁶ But is it not correct to insist that at some point this distinction between essential classes and inessential classes or strata be subjected to empirical rather than logical proof? In his recent attempt to reconcile Marxist theory with the persistence of intermediate classes, Dale L. Johnson wrote that “historical/empirical research must be carried out within the premise that middle-class formation is an *expression* of the class polarization process.” Must it indeed? Is it necessarily the case that class polarization is the essence of capitalist development and that any evidence of the formation or continuing vitality of intermediate classes “must” be subordinated to that deeper reality? Marxists can, I believe, fairly propose a long developmental perspective that will permit observation of the eventual disappearance of transitional and inessential classes, but in the meantime they can only assert that a two-class model for describing past and present capitalist society is something more than a necessity for their own theoretical consistency. And perhaps we might note that, if Johnson is deprived of his premise, there remains in his discussion the interesting proposition that the American “petty bourgeoisie,” “formed . . . in premonopoly stages of capitalist development,” was “a social class of major social weight” throughout the nineteenth century.²⁷

It should also be noted that the concept of middle-class formation has its theoretical advocates, some of whom have worked wholly or partly within the Marxist tradition. Johnson, even while insisting on the transitory character of the middle class, did after all describe it as a class, not as a stratum or a collection of

²⁵ For an excellent discussion of Marxism as a theory of capitalist development, see Isaiah Berlin, *Karl Marx: His Life and Environment* (4th edn., Oxford, 1978), esp. 89–116.

²⁶ Johnson, *Class and Social Development*, 22; and Michael B. Katz et al., *The Social Organization of Early Industrial Capitalism* (Cambridge, Mass., 1982), 39.

²⁷ Johnson, *Class and Social Development*, 106, 105, 200.

"contradictory class locations," and conceded its importance as a class in the nineteenth century. Others follow a similar line of thought: Nicos Poulantzas more abstractly in *Classes in Contemporary Capitalism*, C. Wright Mills more concretely in *White Collar*.²⁸ And there are still others who, beginning with Marxist categories, define intermediate classes without bothering either to subordinate them to more basic classes or to predict their eventual dissolution. Stanislaw Ossowski, for example, found within the basic structure of Marxist theory the necessity of at least one intermediate class, for Marx specified two basic dichotomous relations—ownership or nonownership of the means of production and employment or nonemployment of hired labor. The "overlapping of two mutually incompatible dichotomous divisions leads to the establishment of at least a third category, and thus a three-term scheme emerges." More specifically, from the very architecture of Marxist theory there emerges an intermediate class consisting "of those who own the means of production but do not employ hired labour."²⁹ What is notable here is not Ossowski's discovery of the independent producer—all Marxists are aware of him—but his discussion of a class of such producers no less clearly linked to the means of production than are the bourgeoisie or the proletariat and, so long as the dual dichotomies remain unaligned, no more likely than they to disappear.

Ossowski's interpretation of Marx is singularly static. More dynamic, and of greater use to the empirically minded historian of the middle class, are Anthony Giddens's attempts to leaven Marxist theories of class development with a Weberian yeast.³⁰ In *The Class Structure of the Advanced Societies*, Giddens maintained the Marxist insistence on "the *explanatory salience* of class as central to the notion of class society." "A class society," he wrote, "is not one in which there simply exist classes, but one in which class relationships are of primary significance to the explanatory interpretation of large areas of social conduct." At the same time, he recognized the crucial problem of implementing this fundamental proposition in the analysis of specific societies, of identifying in specific ways "the *modes in which* 'economic' relationships become translated into 'non-economic' social structures." Marxism by itself seems inadequate to the task, and Giddens offered instead a broader method for making the transition from the abstract theory of class to concrete descriptions and analyses of what he called "the *structuration* of class relationships" in the real world. Giddens first distinguished between "mediate structuration," by which he meant the degree of "mobility closure" that provides for—or, in the case of highly mobile, "open" societies, fails to provide for—"the *reproduction* of common life experience over the generations," and three forms of "proximate structuration" that "condition or shape class formation" in more immediate ways. These three are the division of labor within productive enterprises, authority relationships within those enterprises (here Giddens seemed to be thinking of large, multitiered corporations and of Ralf Dahrendorf's objection to the Marxist focus on legal

²⁸ Poulantzas, *Classes in Contemporary Capitalism*, esp. 193–208, 285–90; and Mills, *White Collar: The American Middle Classes* (New York, 1951), esp. 3, 6.

²⁹ Ossowski, *Class Structure in the Social Consciousness* (London, 1963), 32–33.

³⁰ Dale Johnson, an orthodox Marxist, has complained about Giddens's "Marxized Weberian perspective. . . . A rather indiscriminate eclecticism in his theoretical position leads to some provocative critiques but does not lend itself to the construction of a coherent theory"; *Class and Social Development*, 24–25 n.

ownership of, rather than effective authority over, the means of production in a corporate economy), and class relations originating in the sphere of consumption, in particular those identifiable “distributive groupings” that arise from common patterns of consumption. To Giddens, it is the degree to which these sources of “proximate structuration” converge or diverge within a more or less mobile society that determines both the clarity and the salience of class in a given society. Where there is an inconsistent relation between type of work, authority, and levels and patterns of consumption, and at the same time a high degree of vertical mobility, the class principle will be weak. Where these relations are consistent within a setting of “mobility closure,” they reinforce each other and reaffirm the significance of class. To Giddens, in short, class is an empirical question—a principle of social organization that can vary in shape and in strength between one society and another. It is not a necessary adjunct to a theory of capitalist development. Even more than Ossowski, moreover, Giddens suggested the probability of discovering in any society more than two classes and even proposed that “*a threefold class structure is generic to capitalist society*,” for this is the consistent pattern of relations between the sources of structuration that Giddens purported to find in the capitalist world, at least in the advanced capitalist world of the twentieth century.³¹

The real usefulness of Giddens’s theory to the historian of the nineteenth-century middle class, however, may lie not so much in his overlapping categories of class-forming experience as in his adjoining discussion of class consciousness. Giddens distinguished between “class consciousness” and “class awareness” in a way that speaks directly to the paradox of a middle-class ideology of social atomism:

We may say that, in so far as class is a structured phenomenon, there will tend to exist a common awareness and acceptance of similar attitudes and beliefs, linked to a common style of life, among the members of the class. “Class awareness,” as I use the term here, does *not* involve a recognition [admission?] that these attitudes and beliefs signify a particular class affiliation, or the recognition [admission?] that there exist other classes, characterised by different attitudes, beliefs, and styles of life; “class consciousness,” by contrast, . . . does imply both of these. The difference between class awareness and class consciousness is a fundamental one, because class awareness may take the form of a *denial of the existence or reality of classes*. Thus the class awareness of the middle class, in so far as it involves beliefs which place a premium upon individual responsibility and achievement, is of this order.³²

I have taken the liberty to suggest a slight emendation of terms in this statement, because it does not seem possible that the members of a class can be aware of the “similar attitudes and beliefs” associated with their “common style of life” without recognizing “that there exist other classes, characterized by different attitudes, beliefs, and styles of life.” But this change of meaning, though not trivial, does not alter the larger purpose of Giddens’s distinction between “consciousness” and “awareness,” which suggests that different types of class perception—including those that do not posit classes as “conflict groups,” as antagonistic interest groups,

³¹ Giddens, *Class Structure of the Advanced Societies*, 132, 105, 107–09, 111, and esp. 177–97. See Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford, 1959).

³² Giddens, *Class Structure of the Advanced Societies*, 111.

or even as significant groups of any kind—can grow out of situations in which class is a social and cultural reality. Moreover, certain kinds of consciousness or awareness may be expected from certain classes, the middle class in particular being the one most likely to express awareness of its common attitudes and beliefs as a denial of the significance of class.

Giddens has provided the historian of the middle class with a theory that no longer subordinates the middle to the polar extremes and no longer requires that a class manifest itself (or even possess the potential for manifesting itself) in an aggressive and self-conscious ideology—in what is usually called “class consciousness.” To the extent that this theory is persuasive, it creates the opportunity for reformulating the hypothesis of middle-class formation in terms of the “structuration” of certain types of social and cultural experience, to which expressions of class consciousness or awareness are related as a matter of secondary importance. But is it persuasive? I believe it is, insofar as it enables us to look for patterns of experience (patterns that might reasonably be called “middle class”) that we would not discover if we thought only of different kinds of social strata or if we focused only on how class manifests itself in consciousness. At the same time, it must be freely admitted that this is a more modest approach to class and class formation. It is a “theory of the middle range” that makes fewer demands and asserts smaller claims than the “grand theory” of Marx and his followers.³³ For this very reason, however, it is, I believe, a more workable and in the long run a more satisfactory response to Ryan’s call for analysis of “middle Americans as a class unto themselves.”

AT ANY RATE, WE MUST NOTE AT ONCE that there is very little evidence from the nineteenth century that the middle class was emerging as a conflict group, or as a self-conscious social group that insisted on its identity as a class. The term “middle class” and nearly equivalent terms, such as “middle classes,” “middle ranks,” and “middling sorts,” appear far less frequently in the documentary record of the nineteenth century than might be supposed, whereas discussions of social polarization appear with increasing frequency as one surveys the records of the second half of the century.³⁴ This is particularly true of descriptions and analyses of the social structure of cities, the supposed seats of middle-class formation. From at least the middle of the nineteenth century, the American city was portrayed frequently as a two-class society consisting only of the very rich and the very poor. In New York City, claimed Junius Henri Browne in 1869, “there are two great and distinct classes of people—those who pass their days in trying to make money enough to live; and those who, having more than enough, are troubled about the manner of spending

³³ See Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill., 1957), 9.

³⁴ I base this statement in part on analyses of systematic samples of *Harper's*, *The Nation*, *The New York Tribune*, various workingmen's newspapers, and autobiographies, which were undertaken by members of my advanced undergraduate seminar, and in part on my own less systematic sampling of a variety of nineteenth-century documents and publications. For brief discussions of the evolution of the term “middle class” in the nineteenth century, see Bledstein, *Culture of Professionalism*, 8–13; and Asa Briggs, “The Language of ‘Class’ in Early Nineteenth-Century England,” in Asa Briggs and John Saville, eds., *Essays in Labour History* (London, 1960), 43–73, “Middle-Class Consciousness in English Politics, 1780–1846,” *Past and Present*, 9 (1956): 65–74.

it.”³⁵ Many others wrote in the same vein, perhaps none more directly than James Dabney McCabe, who claimed in 1868 that “there are but two classes in the city—the poor and the rich.”³⁶ Recently, Michael Katz, Michael Doucet, and Mark Stern have seized on statements similar to Browne’s and McCabe’s as evidence supporting a two-class model of early industrial society in North America. “On that the evidence appears unequivocal. A two-class model describes not only the objective relations of early industrial cities but the perception of their principal interpreters as well.”³⁷ Can this proposition be refuted by the middle-class historian if contemporary observers spoke so clearly about a two-class society?

Indeed it can, once we understand these observers in the context of a sensationalist literary tradition, extending at least as far back as the *roman-feuilleton* of the 1840s, that impelled them to portray the big city as a shockingly *abnormal* collection of the rich and the poor and to exaggerate the extent of the departure of the big city from traditional communities of middling folk and more modest extremes by exaggerating the polarization of urban society.³⁸ The two-class city (or factory town) was presented as an anomaly, not as a microcosm of a two-class society, though observers sometimes noted (with fear or with enthusiasm) that it was a portent of American society in generations to come.³⁹ And it was exaggerated. Did nineteenth-century writers really believe that there were no intermediate classes in the city? In the sentence immediately following his description of New York as a city of “the poor and the rich,” McCabe wrote, “The middle class, which is so numerous in other cities, hardly exists at all here.” But a closer look reveals that what McCabe really meant was that high housing costs had driven the middle class to the suburbs, not that the metropolis as a whole contained no middle class.⁴⁰ And note that McCabe was able to use the term “middle class” in a popular text with neither quotation marks nor accompanying definition, and he was willing to concede the presence of a middle class in cities other than the one whose “secrets” he had set out to betray. To be sure, McCabe and the other observers of the big city

³⁵ Browne, *The Great Metropolis: A Mirror of New York* (Hartford, Conn., 1869), 23.

³⁶ Edward Winslow Martin [James Dabney McCabe], *The Secrets of the Great City: A Work Descriptive of the Virtues and Vices, the Mysteries, Miseries, and Crimes of New York City* (Philadelphia, 1868), 38. For a fuller discussion of Browne’s and McCabe’s descriptions of New York, see my “Explaining the New Metropolis: Perception, Depiction, and Analysis in Mid-Nineteenth-Century New York City,” *Journal of Urban History*, 11 (1984): 9–38.

³⁷ Katz et al., *Social Organization of Early Industrial Capitalism*, 25. The “objective relations” to which this statement refers consist of allegedly strong relationships between class and wealth, as demonstrated by the extent to which a two-class occupational model explains variation in assessed property in Hamilton and in the value of dwellings in Buffalo. But the authors’ analysis of these relationships is seriously flawed. Their own calculations revealed that a two-class model explained only 13 percent, 3 percent, and 18 percent of the variation in assessed wealth in Hamilton in 1851, 1861, and 1871 respectively, and only 9 percent of the variation in dwelling values in Buffalo in 1855. The fact that a sixteen-category occupational model explained only a small amount of additional variation in two of the four instances led them to conclude that “the majority of the variation in economic rank attributable to occupation was in fact accounted for by class.” This statement may be challenged on at least two grounds. First, the fact that the two-class model explained most of the variation “attributable to occupation” means very little, except that “class” explained *even less* than the modest amounts of variation explained by the sixteen-category model. Second, in two of the four calculations (Hamilton, 1851 and 1861), it explained significantly less, in one case even falling short of the “majority” the authors claimed. See *ibid.*, 47–49.

³⁸ Blumin, “Explaining the New Metropolis.”

³⁹ See, for example, Browne, *Great Metropolis*, 697–700.

⁴⁰ McCabe, *Secrets of the Great City*, 38.

were genuinely impressed by the extremes of wealth and poverty they found there. But in the very act of focusing on these extremes as evidence of a new sort of society, they at once distanced themselves from that society and identified with an older one in which the extremes were less obvious or important than the middle class to which these writers did in fact belong.⁴¹ It may not be too much to say that they were spokesmen for that endangered middle and that their descriptions of the polarized city are evidence not so much of the emergence of a two-class society as of their "awareness" of membership in the middle class.

A few writers of the mid-nineteenth century attempted to identify and describe a "middle class" in more explicit terms. Interesting examples may be found in several of the highly popular descriptive accounts of the emerging metropolis written in the 1850s by a *New York Tribune* city reporter, George G. Foster. In *New York by Gas-Light: With Here and There a Streak of Sunshine*, Foster followed an extended description of the dissipations of rich and poor New Yorkers with an enthusiastic description of what he called "the great middle class," that part of the city's population "whose aspirations, reaching the full standard of well-to-do content, wisely fall short of that snobbish longing after social notoriety which so many of their class mistake for exclusiveness and aristocracy." Does this suggest a new social class, characterized by an unprecedented combination of affluence and the rejection of aristocratic aspirations and manners? Unfortunately, Foster did not pursue this idea and, in his several evocations of the "great middle class," developed no consistent definition or terminology. There is even one reference to a "great middle working class."⁴² Foster did seem to be groping toward a new way of describing the intermediate classes or strata of the mid-nineteenth-century city and at one point hit on a definition that accords quite well with our own sense of what the middle class consists of. But his experiment in the social description and taxonomy of the middle class is ultimately confused and incomplete, as are those of his contemporaries.⁴³ It is interesting to note that the first edition of John Russell Bartlett's *Dictionary of Americanisms*, published in 1848, contains no entry entitled "middle class." Neither does the second edition, published eleven years later, but it does include an entry entitled "Middling Interest"—defined as "the middle class of people."⁴⁴

Even by the end of the nineteenth century few American writers had explicitly identified a "middle class" to which they or anyone else belonged, though the term was occasionally used without definition or explanation.⁴⁵ If this and the above

⁴¹ Most were professional writers, journalists, clergymen, or public officials.

⁴² Foster, *New York by Gas-Light: With Here and There a Streak of Sunshine* (New York, 1850), 69, *New York in Slices: By an Experienced Carver; Being the Original Slices Published in the New York Tribune* (New York, 1849), 72, and *New York Naked* (New York, n.d.), 115, 146. I discuss Foster's works in detail in my "Explaining the New Metropolis."

⁴³ See, in particular, the quotation from William Ellery Channing in Briggs, "The Language of 'Class,'" 48; and Bledstein, *Culture of Professionalism*, 8–13.

⁴⁴ Bartlett, *Dictionary of Americanisms: A Glossary of Words and Phrases Usually Regarded as Peculiar to the United States* (1st edn., New York, 1848; 2d edn., Boston, 1859), 270.

⁴⁵ Bledstein claimed that the first dictionary definition of "middle class" appeared in *The Century Dictionary* in 1889; *Culture of Professionalism*, 13. One example of how the term "middle class" could be used and defined in a political speech is the following extract from Hazen Pingree's address to a conference on trusts in 1900: "The

discussion suggests a degree of middle-class "awareness," it does not suggest any significant degree of middle-class "consciousness," and any hypothesis of middle-class formation that requires evidence of the latter certainly will not be confirmed in the setting of nineteenth-century American society. But Giddens, as we have seen, has pointed the way to the reformulation of a more limited version of this hypothesis, one that looks primarily for new convergences of personal and social experience among intermediate strata. Did significant aspects of the daily lives of middling folk change during the nineteenth century in ways that indicate the formation of a new, relatively coherent, and relatively clearly bounded (if not self-conscious) social group, located in an intermediate position within a hierarchically organized social structure? More simply put, can we identify an emerging middle-class way of life in the nineteenth century? What were the specific components of that way of life?

Giddens supplied a specific list, drawn from the most useful insights of Marx, Dahrendorf, and Weber, but I do not propose to follow his eclectic model except as a point of departure for a still more inclusive approach to the "structuration" of middle-class experience. I believe I can describe those aspects of personal and social life most critical to an experiential hypothesis of middle-class formation and can assess, on the basis of the limited research already conducted by me and by others, the likelihood that such a hypothesis will be confirmed. I can also suggest ways in which essential additional research might be carried out. Briefly stated, the experiences I have in mind are those that involve work, consumption, residential location, formal and informal voluntary association, and family organization and strategy.

Work, here as everywhere, is the starting point. At the heart of nearly every attempt to define an emerging middle class lie propositions concerning the increasing association of "nonmanual" (eventually "white collar") occupations with membership in a class or classes located hierarchically above the "manual" (eventually "blue collar") working class. More specifically, these propositions involve the increasing association of small-scale, usually nonmanual entrepreneurship (the petty bourgeoisie) and salaried, nonmanual employment with an intermediate or lower middle class located hierarchically between the working class and a class of wealthy entrepreneurs and controllers of capital called variously the upper class, the upper middle class, or the haute bourgeoisie. Underlying the new alignments between occupations and social class were specific but often poorly understood changes in work itself, and to comprehend the relationship between work and middle-class formation we must look beyond and beneath mere occupational labels to changes in the character of work of various kinds—to the emergence of new tasks and the reorganization of old tasks—and to the quantitative expansion of

real strength of our republic . . . has always been in what is called our middle class, . . . manufacturers, jobbers, middle men, retail and wholesale merchants, commercial travelers and business men generally. . . . Close to them . . . are the skilled mechanics and artisans . . . the sinew and strength of the nation." Pingree, as quoted in Rowland Berthoff, "Independence and Enterprise: Small Business in the American Dream," in Stuart W. Bruchey, ed., *Small Business in American Life* (New York, 1980), 35. Note in Pingree's definition that skilled blue-collar workers are "close to" but distinct from a middle class that is defined entirely in terms of nonmanual work.

certain forms of work relative to other forms. We must also examine the changing *environments* of work—the increasing physical segregation of certain types of work from other types, the increasing cleanliness and refinement of one workplace rather than another, and the changing scale of workplaces of various kinds. We must, of course, examine changing relationships between types of work and authority and changing cultural meanings (especially those relevant to social status) attached to work of different kinds. Only then can we attempt to associate work with occupational labels and occupational labels with changes in the structure of social classes. How, then, did changes in work, and associated changes in occupations, contribute to middle-class formation in the nineteenth century?

This is a very broad question that historians have only begun to answer. Yet a few significant observations can be made with some confidence. First, several types of nonmanual tasks did seem to change in important ways, and to expand relative to work as a whole, during the course of the nineteenth century. Most obvious is the expansion and specialization of clerical work in the last decades of the century in association with the multiplication of large-scale, complex, departmentalized business firms of various kinds. The number of office workers (bookkeepers, cashiers, accountants, office clerks, stenographers, and typists) increased ninefold in the last thirty years of the century, from fewer than seventy thousand to more than six hundred thousand, and, though fully one-third of this increase is accounted for by the entrance of women into all types of clerical office work, it is nonetheless true that the multiple of increase for men was nearly three times as great as the multiple for the male work force as a whole.⁴⁶ Male store clerks tripled in number between 1870 and 1900, a multiple half again as large as that for the entire male work force.

Associated with this expansion of the “white collar” work force were certain changes in the character and environment of clerical work, some of which were visible well before 1870, especially in the larger cities. Specialization and gradation of tasks in an office environment physically removed from production and other forms of manual work were introduced as early as the end of the eighteenth century by insurance companies, banks, and merchants’ and stock exchanges and were extended into new areas by some of the larger transportation and shipping companies, newspaper offices, and manufacturers of the antebellum era. Wholesalers, and even a few retailers, were learning to set office work apart from the handling of merchandise, and it is likely that these firms formed part of the market for the office furniture and supplies that began to appear in some city directory advertisements during the 1850s.⁴⁷ Meanwhile, parallel changes were occurring in the selling of goods. Specialization and the physical segregation of sales personnel occurred at a number of levels, from large department stores (dating from A. T. Stewart’s “Marble Palace” of 1846) to the increasingly numerous and elegant

⁴⁶ Alba M. Edwards, *Comparative Occupation Statistics for the United States, 1870 to 1940* (Washington, D.C., 1943), tables 8–10, pp. 104–29. For a similar tabulation, see Margery Wynne Davies, “Woman’s Place Is at the Typewriter: The Feminization of Clerical Workers and Changes in Clerical Work in the United States, 1870–1930” (Ph.D. dissertation, Brandeis University, 1979), 187–89. Davies argued that clerical workers were increasingly proletarianized as they came to work at machines in large, impersonal offices. This argument is probably more accurate for women workers than for men.

⁴⁷ See, for example, *The Boston Directory for the Year 1855* (Boston, 1855).

specialized retailers, and increasingly specialized wholesalers, to manufacturers who more and more frequently built wholesale or retail outlets in locations separate from their factories.⁴⁸ William D. Rogers, the Philadelphia coach maker, for example, whose factory was at Sixth and Masters in the northern district of Penn Township, sold his products during the 1850s from an elegant "carriage repository" at the downtown location of Tenth and Chestnut. (An engraving of the interior of the "repository" reveals a separate room or rooms designated as the "office.")⁴⁹ David Felt and Company manufactured stationery and other paper products in New Jersey in the 1840s, sold them wholesale at Pearl Street, New York, and retail at Wall Street, as well as at a retail branch in New Orleans.⁵⁰ When Madame Demorest moved her New York dress pattern business from three separate buildings to a single building on Fourteenth Street in 1874, she did not contradict this trend, for she maintained clear distinctions between the showroom and sales desks on the ground floor, the offices and editorial rooms on the second floor, and the workrooms above.⁵¹ By this time there were hundreds of such showrooms, retail outlets, and "repositories" and thousands of independent retail stores, where store clerks, trained and dressed for "head work" rather than "hand work," sold goods in clean and even elegant environments, well removed from the dirt, noise, and personnel of the workshop.

Essentially the same point may be made about many of their employers. The independent, specialized retailer was a product of the nineteenth century, an offshoot of changes in the industrial and commercial sectors that more and more frequently removed the once-independent artisan and the once-general importer from direct contact with the consuming public. At the beginning of the century most domestically produced goods were sold to the public by the men who made them, and many imported goods were sold at retail by merchants who brought them from abroad. But during the middle decades of the century, as artisans became increasingly confined to the role of contractor or even wage-earning producer for merchant capitalists and manufacturers and as importers and other large-scale merchants confined themselves increasingly to wholesaling, a vacuum was created at the retail level that was filled in part by independent retailers—men who had nothing to do with production, shipping, or wholesaling. Many were former artisans, no doubt, and some of these did not entirely banish production from their premises, but in all cities, large and small, there emerged large numbers of men whose primary or sole economic function was retailing.⁵² Most were

⁴⁸ See Harry E. Resseguie, "Alexander Turney Stewart and the Development of the Department Store," *Business History Review*, 39 (1965): 301–22, "A. T. Stewart's Marble Palace: The Cradle of the Department Store," *New-York Historical Society Quarterly*, 48 (1964): 131–62; Ralph M. Hower, *History of Macy's of New York, 1858–1919: Chapters in the Evolution of the Department Store* (Cambridge, Mass., 1943); Stuart M. Blumin, "Black Coats to White Collars: Economic Change, Nonmanual Work, and the Social Structure of Industrializing America," in Bruchey, *Small Business in American Life*, 100–21; Glenn Porter and Harold C. Livesay, *Merchants and Manufacturers: Studies in the Changing Structure of Nineteenth-Century Marketing* (Baltimore, 1971); and Thomas C. Cochran, "The Business Revolution," *AHR*, 79 (1974): 1449–66.

⁴⁹ Edwin T. Freedley, *Philadelphia and Its Manufactures*. . . (Philadelphia, 1858), 443–47.

⁵⁰ *Andrews and Co's Stranger's Guide in the City of New-York* (New York, 1849), 18.

⁵¹ Margaret Walsh, "The Democratization of Fashion: The Emergence of the Women's Dress Pattern Industry," *Journal of American History*, 66 (1979–80): 309.

⁵² According to one estimate there were some 7,000 retailers in New York City in 1831; Ray Bert Westerfield,

specialists in particular lines of goods, and it is fairly clear that their new economic role was not only more distinctive but also more professional than that of the more generalized and less consequential storekeepers of earlier generations. Succeeding to some of the respect that traditionally accrued to the master craftsman as an expert in his line, the retailer also began to acquire the respect that attached to the man of commerce. It is in the Jacksonian era that we begin to see the phrase "retail merchant."⁵³

Office clerks, store clerks, managers of retail outlets, and independent retailers were in many ways new figures in the nineteenth-century economy and in the expanding "white collar" work force, as were the large but probably indeterminable numbers of master craftsmen who withdrew from direct participation in production in order to organize and supervise the manual work of others, either in their own workshops or in the shops or homes of other artisans or less skilled workers. These men are best thought of as contractors, managers, manufacturers, or manufacturers and retailers, rather than as craftsmen, for their work, their role, and their relationships to workers and customers were no longer those of the traditional master. The point is obvious; less obvious is how these manufacturers, contractors, or manufacturing retailers can be located and tabulated in the historical record. Most continued to list themselves in city directories and on censuses as craftsmen, as did producing masters and even journeymen. The potential for distortion of the structure of work and occupations, or of the circumstances of any one individual, by an uncritical acceptance of the artisanal labels that come down to us from the nineteenth century is, therefore, enormous. Consider two examples. The advertisement for Edward Fox's tailoring and clothing business in *Doggett's New York City Directory for 1850-51* boasts a custom department, a cutting department (employing "none but the most experienced artists" and "none but the most thorough workmen"), a ready-made clothing department, and a furnishings department, offering neckties, shirts, scarves, gloves, and other items. It is obvious that Fox himself was a manufacturer and retailer of clothing, a man who organized and managed a fairly large local business. It is possible, but not likely, that he combined with his managerial functions work with needle and shears. In any case, he is listed in the body of the directory simply as "tailor," without distinction from the ordinary master tailor or even from some of his own employees.⁵⁴ Even more striking is the example of Cyrus Alger, a "founder" in *The Directory of the City of Boston for 1850*. Alger was in fact coproprietor of the South Boston Iron Company, which operated a large iron foundry in South Boston from an office on Central Wharf.⁵⁵ He was not a skilled worker but an industrial

"Early History of American Auctions—A Chapter in Commercial History," *Transactions of the Connecticut Academy of Arts and Sciences*, 23 (1920): 184. The 1855 New York census lists 9,617 retailers in New York County; *Census of the State of New York for 1855* (Albany, 1857), 479. By 1870 there were more than 400,000 retailers in the United States; Edwards, *Comparative Occupation Statistics for the United States, 1870 to 1940*, 110.

⁵³ See, for example, Edward Hazen, *Popular Technology; or, Professions and Trades*, 1 (New York, 1844): 188-89. For a brief but interesting description of a mid-nineteenth-century American retail store, see [Isabella Lucy Bird Bishop], *The Englishwoman in America* (London, 1856), 340.

⁵⁴ *Doggett's New York City Directory for 1850-51* (New York, 1850).

⁵⁵ *The Directory of the City of Boston* (Boston, 1850).

capitalist. What these examples tell us is that the nonmanual sector of the mid- and late nineteenth-century urban economy was probably much larger than is generally estimated and that the continued use of traditional artisanal labels through most of the century disguises a wide variety of economic circumstances and experiences, including those of the "artisan" who no longer worked with his hands.⁵⁶

All of these changes in the character and the physical and institutional environments of nonmanual work suggest an increasing divergence between nonmanual and manual work, not only as a day-to-day experience but also as a foundation for the development of more distinct social classes. "The new discipline of urban work," wrote Sam Bass Warner, Jr., "harassed and enticed Americans into a permanent acceptance of a social division between hand workers and pen wielders, operatives and clerks, the blue collar and the white."⁵⁷ There is a fair amount of evidence to support this statement, not merely in the many casual references we find to "headworkers" and "handworkers" but, more compellingly, in the written advice addressed to nonmanual or manual workers that assumes or articulates the fundamental distinction between them as social as well as economic groups.

An interesting example from the antebellum period is the advice offered to both mechanics and clerks, in separate publications, by the clergyman James Waddell Alexander. Alexander's two-volume *The American Mechanic and Working Man* is notable not because it specifies the distinction "between head-work and hand-work" as the one "usually employed" to define the workingman as a separate social category but because it confirms that view of the social structure even while proposing to redefine as workingmen all workers, manual or nonmanual, who make a useful contribution to society. For the latter purpose Alexander compared bookkeepers to carters, mill directors to operatives, and architects to hod carriers, but, as the very structure of the comparison suggests, he succeeded only in underscoring the distinctions between them. Bookkeepers, mill directors, and architects, moreover, make only the briefest appearance in these volumes, which were intended, after all, for the "apprentice, the journeyman, and the master mechanic." More important, these distinctly manual workers are addressed throughout with a striking degree of condescension. Alexander's principal message to the mechanic was that he content himself with a humble life of manual work and simple domestic pleasures and that he avoid working too hard ("he who overtasks his days has no evenings"), aspiring to wealth, or joining a union⁵⁸—that he live, in short, as a virtuous and contented *mechanic*. Even more condescending than this

⁵⁶ See Blumin, "Black Coats to White Collars," 104–05. Most studies of occupational mobility in the nineteenth century have too readily accepted surviving occupational labels as indicative of the work experiences and economic circumstances of the individuals to whom those labels are attached in city directories and census manuscripts. The only significant exception is Clyde Griffen and Sally Griffen's *Natives and Newcomers: The Ordering of Opportunity in Mid-Nineteenth-Century Poughkeepsie* (Cambridge, Mass., 1978). On the artisanal origins of particular manufacturers, see Susan E. Hirsch, "From Artisan to Manufacturer: Industrialization and the Small Producer in Newark, 1830–60," in Bruchey, *Small Business in American Life*, 80–99; and Herbert G. Gutman, "The Reality of the Rags-to-Riches 'Myth': The Case of the Paterson, New Jersey, Locomotive, Iron, and Machinery Manufacturers, 1830–1880," in Thernstrom and Sennett, *Nineteenth-Century Cities*, 98–124.

⁵⁷ Warner, *The Urban Wilderness: A History of the American City* (New York, 1972), 77.

⁵⁸ Alexander, *The American Mechanic and Working Man*, 2 vols. (New York, 1847), 2: 215–19, 9–44, 55–107, 224–37, 202–14, 188–96, 200, 115, 123, 125, 220–23; 1: 2, 8–10, 24–34, 161–274, 16–19, 105–12, 146–47, 134–35. Volume 1 of this work was first published in 1838 under the pseudonym Charles Quill.

message is the tone and form of Alexander's volumes. Much of *The American Mechanic* is written in homey, playful, even childish little sketches, in which wise mechanics, speaking mainly in proverbs, display a humble philosophy of simple contentment and virtue.

"There is something in the homespun philosophy of *Uncle Benjamin* which always secures my attention," explained the author. "Rude as it is, it has that strength which is often wanting in schools and books. Uncle Benjamin has never read Lord Chesterfield, and, therefore, has not learned how exceedingly vulgar it is to use a common proverb." More candidly, "In the belief that the common mind in every age is best reached by parables, I have sometimes indulged in a little fiction."⁵⁹ This condescension to "the common mind" is not found in Alexander's essay addressed to clerks. The advice, moreover, is very different. As a prospective businessman, the clerk should work hard and prepare himself not only for business but also for a high position in society. He should grow rich in learning, in part so that he can converse with a "brilliant wife."⁶⁰ He will come to occupy, in short, a social world entirely separate from that of the humble mechanic. Indeed, he already does. In a Scottish edition of his essay, Alexander explained in a footnote the American use of the word "clerk" to his readers in the British Isles. "In America it is customary to denominate as 'clerk' all young men engaged in commercial pursuits. . . . Of course, the term is not applied to those engaged in mechanical trades."⁶¹

This distinction between commercial pursuits and mechanical trades had become quite commonplace by the middle of the nineteenth century. Later it would evolve into a "white collar-blue collar" dichotomy that, though common only in the twentieth century, was sufficiently advanced by 1869 to inform a description of the succession of classes on the morning New York ferries, where "mechanics with their flannel and check shirts" were followed after seven o'clock by salesmen, accountants, and clerks, "and the shirts of the passengers begin to whiten and raiment to improve."⁶² At first glance these actual and perceived divergences between manual and nonmanual workers would seem to support the two-class model of early industrial society proposed by Katz, Doucet, and Stern. But a closer look suggests that the "business class" was far from being a homogeneous entity and that there were real and identifiable subdivisions of occupational experience within the "white collar" world, which contributed to the formation of distinct intermediate classes. Katz, Doucet, and Stern considered this point with reference to "a distinct stratum

⁵⁹ *Ibid.*, 1: 105, 80. The childlike tone of Alexander's sketches is very striking. I could not help noticing the similarity of construction and tone in the following sentences. "I regret to say that Sammy is sadly destitute of thrift"; *ibid.*, 1: 105. "I am sorry to say that Peter was not very well during the evening"; Beatrix Potter, *The Tale of Peter Rabbit* (New York, n.d.), 56.

⁶⁰ Alexander, "The Merchant's Clerk Cheered and Counseled," in *The Man of Business, Considered in His Various Relations* (New York, 1857), 15–20, 26–27, 35–45, 42–43.

⁶¹ Alexander, "The Young Man of Business Cheered and Counseled," in *The Man of Business Considered in Six Aspects: A Book For Young Men* (Edinburgh, 1864), 13. Note the difference in the title of Alexander's essay in note 60, above, and see the essay by John Todd, "Men of Business, Their Position, Influence, and Duties," in the same volume. For a different view, which places "mechanics" within the middle class, see Gilkeson, "City of Joiners," 1–48. Gilkeson's documents, however, consist mainly of political and reform society speeches. In my view these speeches express a majoritarian political rhetoric that did not necessarily accord with the social perceptions and assumptions of either the speaker or his audience.

⁶² Browne, *Great Metropolis*, 103–04.

of marginal, petty producers" who tended to shift back and forth between self-employment and wage work without becoming either as well-to-do as stable masters or as poor as stable wage earners.

If these marginal producers are safely dismissed as the dwindling survivors of a precapitalist mode of production,⁶³ the same cannot be said of the nonmanual employees and small-scale or medium-scale entrepreneurs I have described above. Clerks, to be sure, continued to be regarded as businessmen in training, not as an enduring occupational group whose social position could be affected by the fact of salaried employment, but there were significant signs, even at the beginning of the era of expanding white-collar employment, that a more permanent clerical work force was in the offing. On the 1870 federal census schedules applying to Philadelphia, for example, 28 percent of those native white males listed as clerks and store clerks, 37 percent of those listed as salesmen, and 55 percent of those listed as bookkeepers were thirty years of age or older—and about half of these were in their forties, fifties, and sixties.⁶⁴ More important, there had emerged by this time a sizable body of small- and medium-scale nonartisanal entrepreneurs who were as distinct from the large-scale capitalists above them as they were from the mechanics beneath them and, unlike the latter, were not in any way a holdover from earlier modes of production. Decades before the emergence of a "new middle class" of technicians, managers, and bureaucrats, therefore, an "old middle class" (the term belongs to C. Wright Mills) may have formed itself in part out of the new work experiences and environments and the new institutional arrangements of the early industrial era.

THE OTHER RELEVANT ASPECTS of personal and social experience must be discussed more briefly, not only because of the constraints of space but also because of the paucity of existing research. This is particularly the case in the very crucial area of consumption. There are many studies of the various consumer goods of the nineteenth century and a number of attempts to compare incomes with living costs for various income strata,⁶⁵ but no one as yet has attempted to examine the actual

⁶³ Katz et al., *Social Organization of Early Industrial Capitalism*, 42–44, and esp. 59, 61. Katz, in *People of Hamilton*, placed all artisans in a distinct middle class but specifically rejected that view in the more recent volume. See Katz, *People of Hamilton*, 311; and Katz et al., *Social Organization of Early Industrial Capitalism*, 44.

⁶⁴ I gratefully acknowledge the indispensable assistance of the director and staff (especially Henry Williams) of the Philadelphia Social History Project in making available to me their machine-readable tapes of these census manuscripts. Of the seven "Native White American" accountants in their sample, only one was less than 30 years of age, and three were 40 or older. Also see Browne, *Great Metropolis*, 104; and Allan Stanley Horlick, *Country Boys and Merchant Princes: The Social Control of Young Men in New York* (Lewisburg, Pa., 1975), 170–71.

⁶⁵ For perhaps the most interesting study of consumer goods, see Siegfried Giedion, *Mechanization Takes Command: A Contribution of Anonymous History* (New York, 1969), 329–476, 512–603, 659–81. For an excellent recent collection of specialized studies, see Thomas J. Schlereth, ed., *Material Culture Studies in America* (Nashville, 1982). Also see Williamson and Lindert, *American Inequality*, 97–132; Jeffrey G. Williamson, "Consumer Behavior in the Nineteenth Century: Carroll D. Wright's Massachusetts Workers in 1875," *Explorations in Entrepreneurial History*, 2d ser., 4 (1967): 98–135; Donald R. Adams, "Wage Rates in the Early National Period: Philadelphia, 1785–1830," *Journal of Economic History*, 28 (1968): 404–26; Eudice Glassberg, "Work, Wages, and the Cost of Living: Ethnic Differences and the Poverty Line, Philadelphia, 1880," *Pennsylvania History*, 66 (1979): 17–58; Michael R. Haines, "Poverty, Economic Stress, and the Family in a Late Nineteenth-Century American City: Whites in Philadelphia, 1880," in Theodore Hershberg, ed., *Philadelphia:*

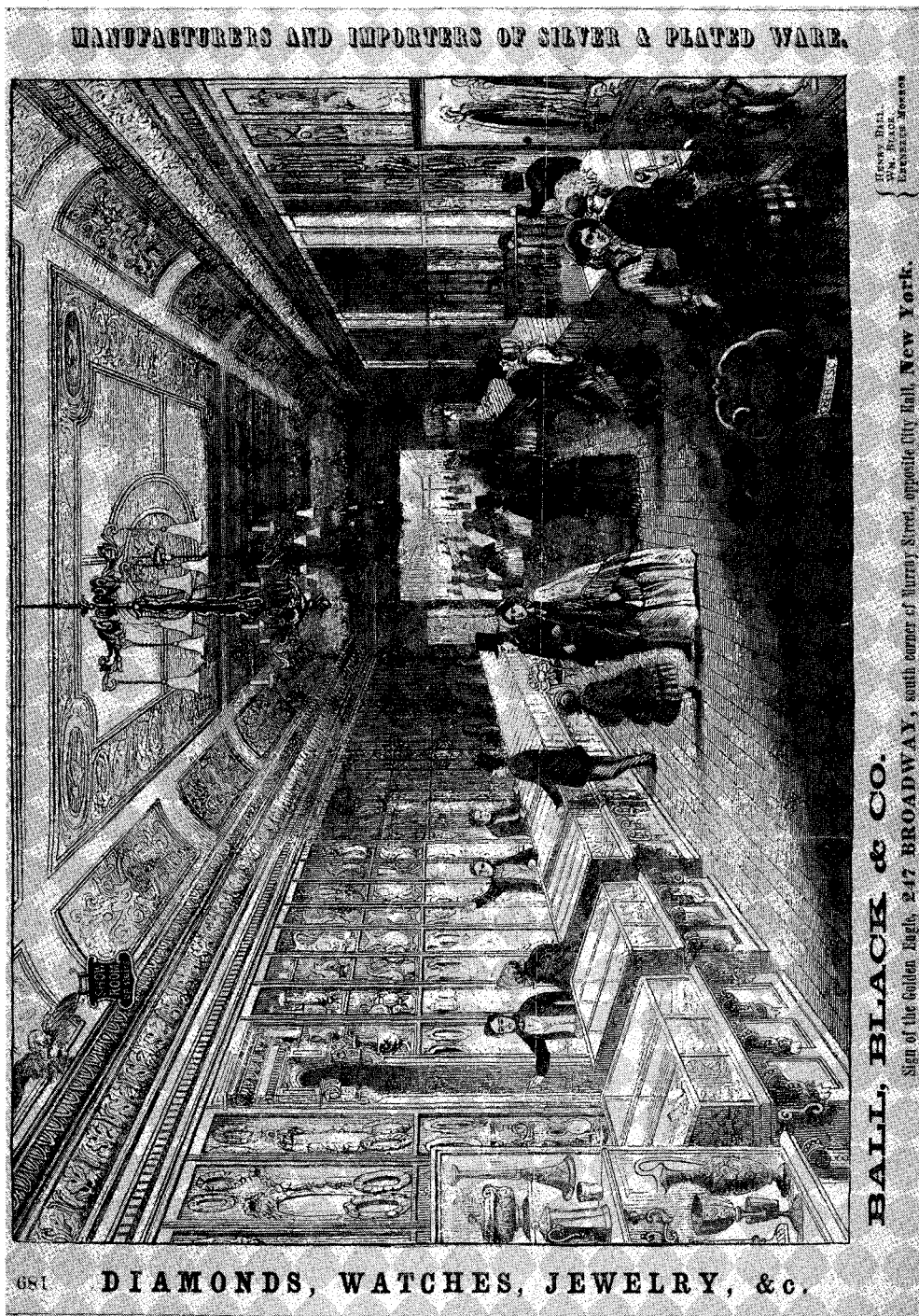
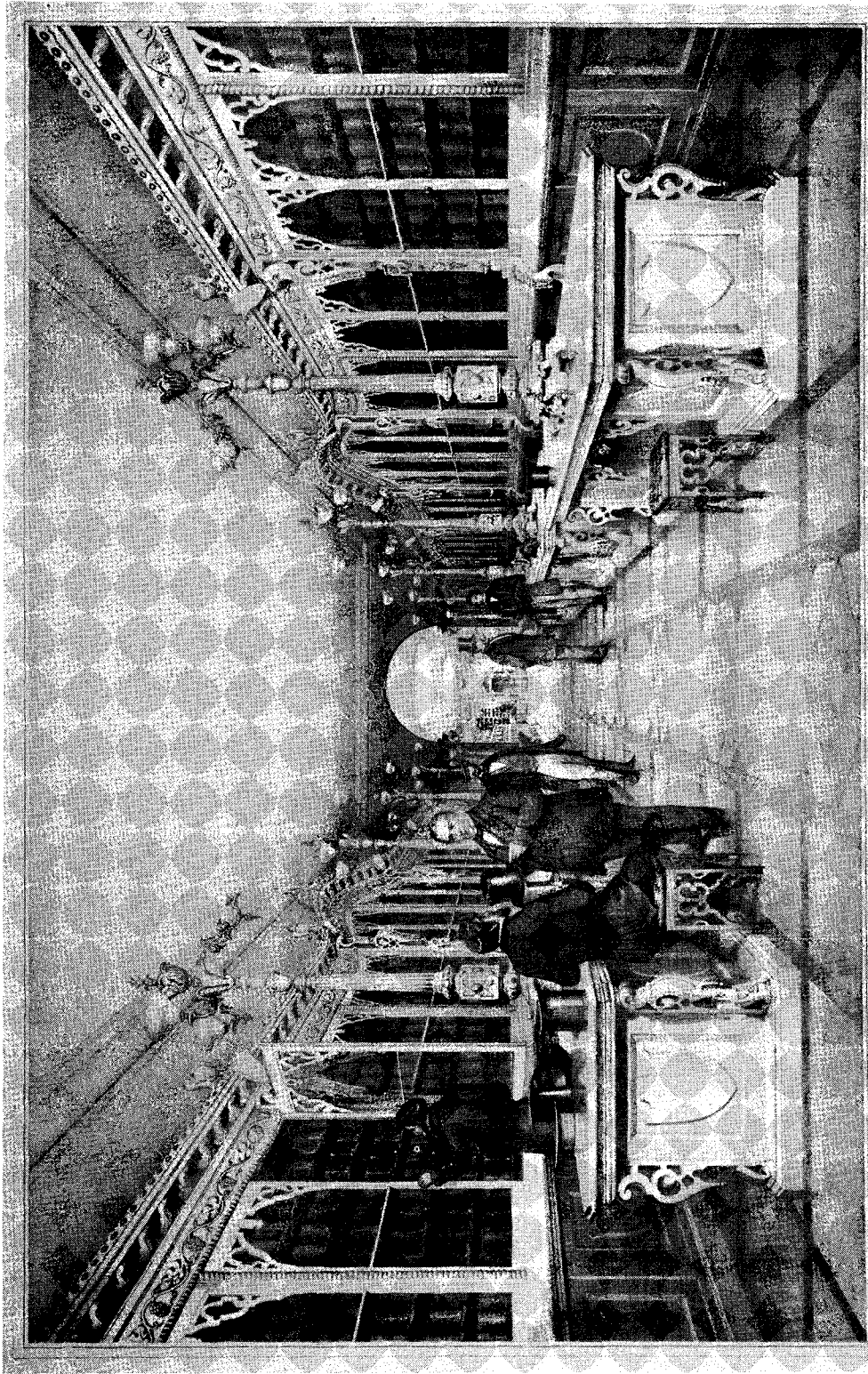


Figure 1: The emergence of elegant retailing. At least thirteen well-attired, male sales clerks are depicted in this mid-1850s city directory advertisement for a New York jeweler. Two of the clerks point to another showroom upstairs. Advertisements depicting the elegant interiors of retail stores first appeared in the 1840s and were common by the 1850s. Engraving reproduced courtesy of the Smithsonian Institution, Washington, D.C., National Museum of American History, Archives Center, Collection of Business Americana.



CHARLES OAKFORD'S MODEL HAT STORE,

159 CECIL STREET, SINGAPORE.
HATS, CAPS & FURS, WHOLESALE & RETAIL.

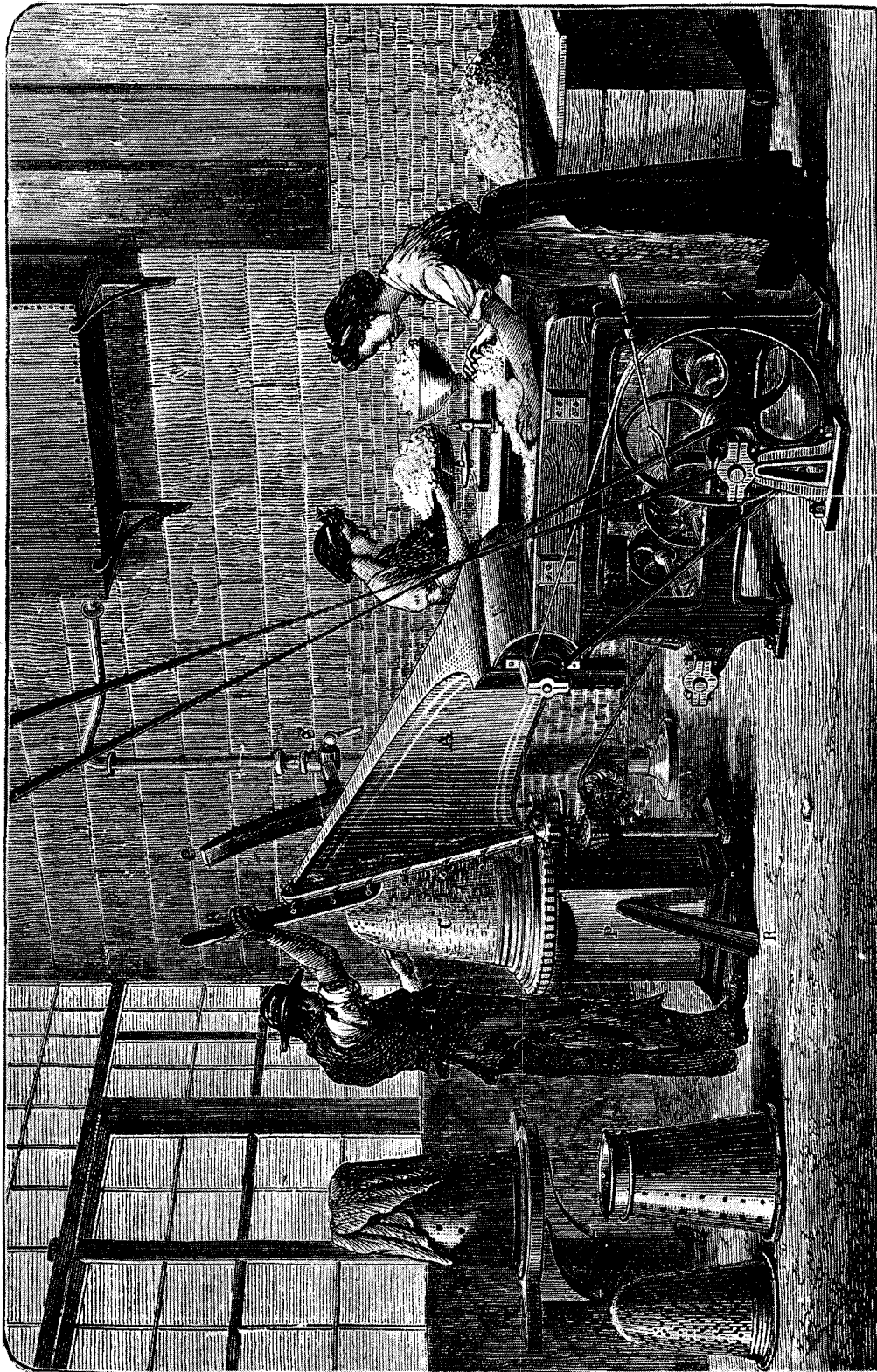
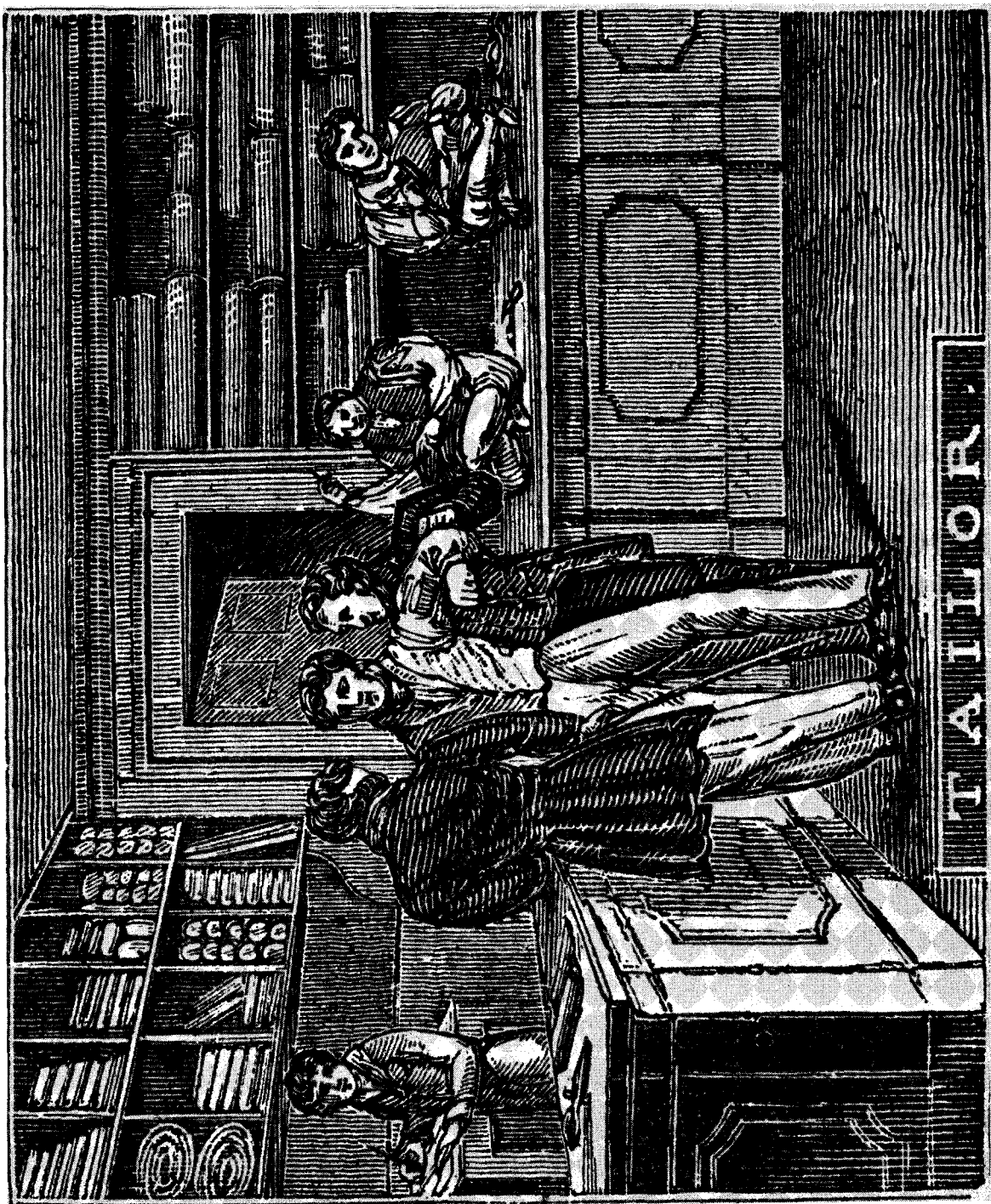


Figure 2: The divergence of nonmanual from manual work environments is conveyed by these contrasting views: *top*, Charles Oakford's Chestnut Street hat store in Philadelphia and, *bottom*, three workers operating a felt-making machine in a hat factory. Engraving, *top*, reproduced courtesy of the Library Company of Philadelphia. Engraving, *bottom*, appeared in the *Scientific American*, January 22, 1876, p. 50, and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y.



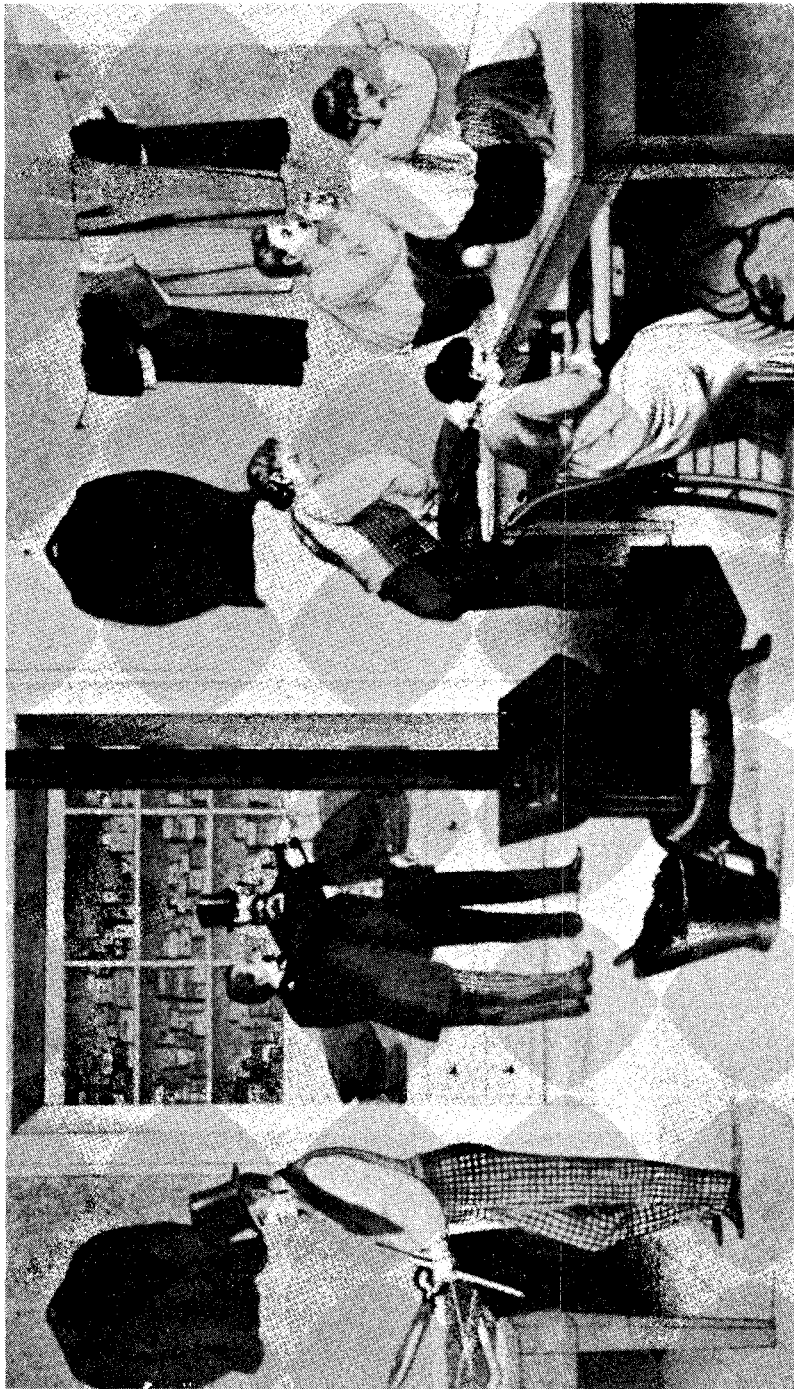


Figure 3: Two views of tailoring. The top illustration depicts an establishment characteristic of a centuries-old tradition in which all the tasks of the tailor's shop are performed in one room. The bottom illustration, one of a series of "Aids for Object Teaching" published in 1874 by Boston lithographer Louis Prang, shows a rather different enterprise. In Prang's tailor shop the manual work of sewing, pressing, and cutting is confined to one room, while the well-dressed master tailor measures a customer in a separate room. Note how Prang used the stove and stovepipe to emphasize the separation of the less skilled workers from the master, while the skilled cutter, who is better dressed than the sewers and presser, stands by himself in the workroom, visually separated from the master by a lightly shaded door frame. Prang depicted several other trades in a very similar manner. Woodcut, *top*, appears in Edward Hazen, *The Panorama of Professions and Trades; or, Everyman's Book* (Philadelphia, 1836), p. 59, and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y. Lithograph, *bottom*, is reproduced courtesy of the Library of Congress, Washington, D.C.



Figure 4: The office environment. The cover illustration of this 1886 office furniture catalogue depicts a quiet, comfortable, and dignified office interior. Framed pictures on the wall and the well-dressed male and female figures lend an air of refined domesticity to a room that is filled not with sofas and pianos but with desks and filing cabinets. Other illustrations in this catalogue highlight polished marble floors and carved ceilings. Engraving reproduced courtesy of the Smithsonian Institution, Washington, D.C., National Museum of American History, Archives Center, Collection of Business Americana.

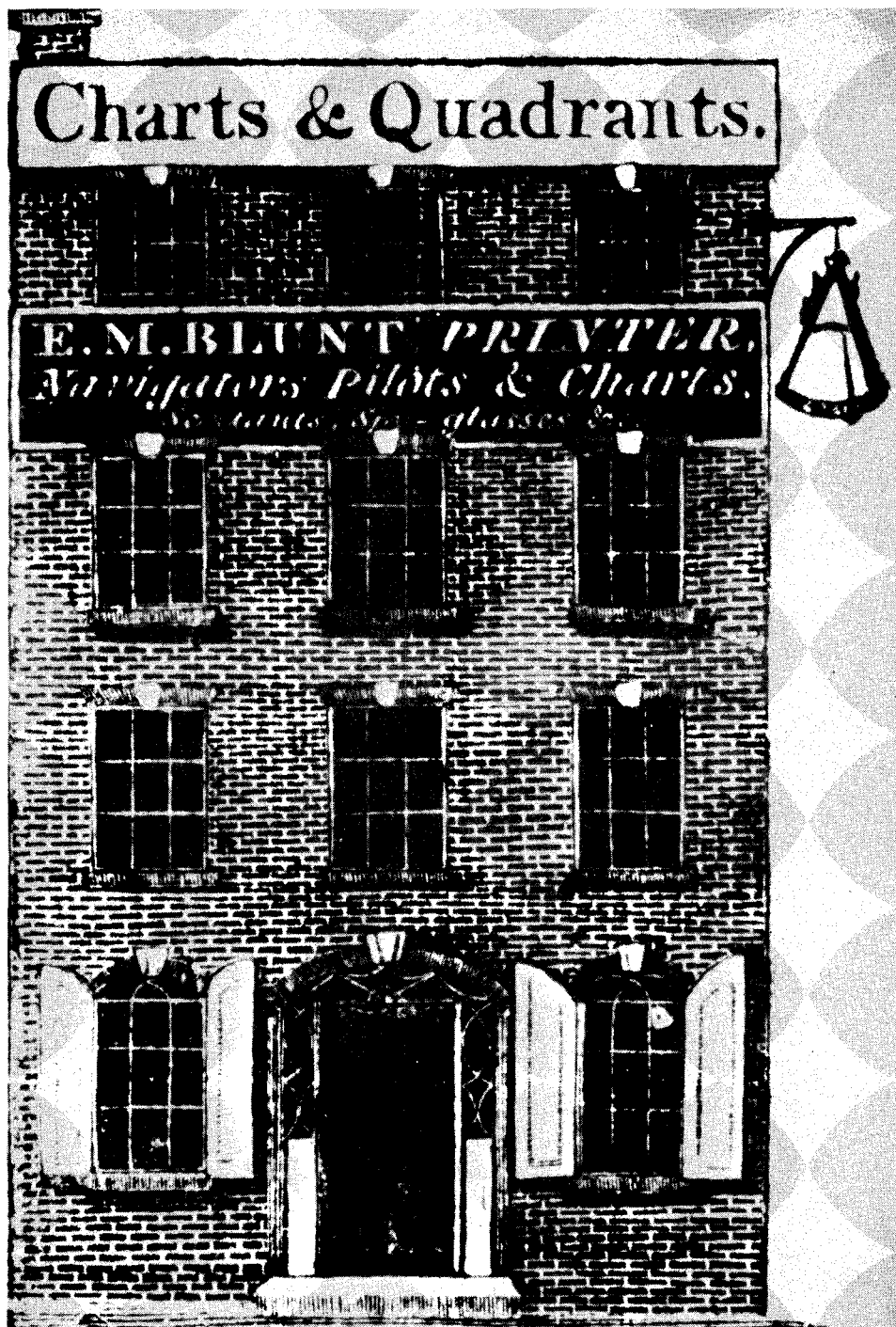
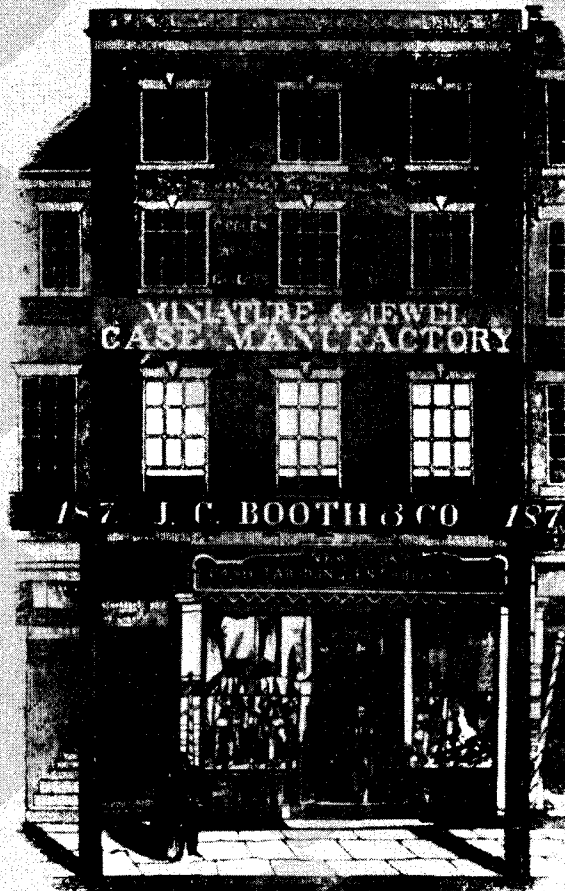


Figure 5: A typical shop exterior of the early nineteenth century from an 1811 city directory advertisement. Before the 1830s many city buildings combined production, trade, and domestic life behind building fronts that did not proclaim any of these as separate activities. The engraving appears in *Longworth's American Almanac* (New York, 1811) and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y.

**NEW-YORK CASH TAILORING ESTABLISHMENT,
AND
GENTLEMEN'S GENERAL FURNISHING STORE,
No. 187 BROADWAY,**

THREE DOORS BELOW THE FRANKLIN HOUSE.

DRESS,
FROCK,
PEL TO
SACK,
AND
OFFICE
COATS,
Over-coats,
AND
CLOAKS,
PANTALOONS
AND
VESTS,
In every Variety
OF
FIGURE
AND
FASHION.



Linen
Muslin, and figured
SHIRTS,
Silk, Merino,
and Cotton Net
UNDER-SHIRTS,
Silk, Merino,
Linen, and Muslin
Drawers,
HOSIERY,
GLOVES,
SUSPENDERS,
Pocket and Neck
Hdkfs.
STOCKS,
Bathing Caps,
UMBRELLAS,
Collars,
Bosoms, &c.

**J. C. BOOTH & CO.
TAILORS:**

*Importers and Dealers in Cloths, Cassimeres, Vestings, and Fancy Articles, of every
description belonging to a Gentleman's Wardrobe.*

Figure 6: The exterior of a "cash tailoring establishment" and "furnishing store" of the 1840s. By the 1830s and 1840s the emergence of retailing was reflected in building fronts. Here, in an older multifunctional building of the sort depicted in Figure 5, a ground-floor retail store displays ready-to-wear clothing in plate-glass windows on either side of a wide doorway. Note also the pilasters and overhead sign that frame the shop front and set it apart from the rest of the building. The columns and lintel at the curb are awning supports. The engraving appears in *Sheldon and Company Business or Advertising Directory* (New York, 1845) and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y.

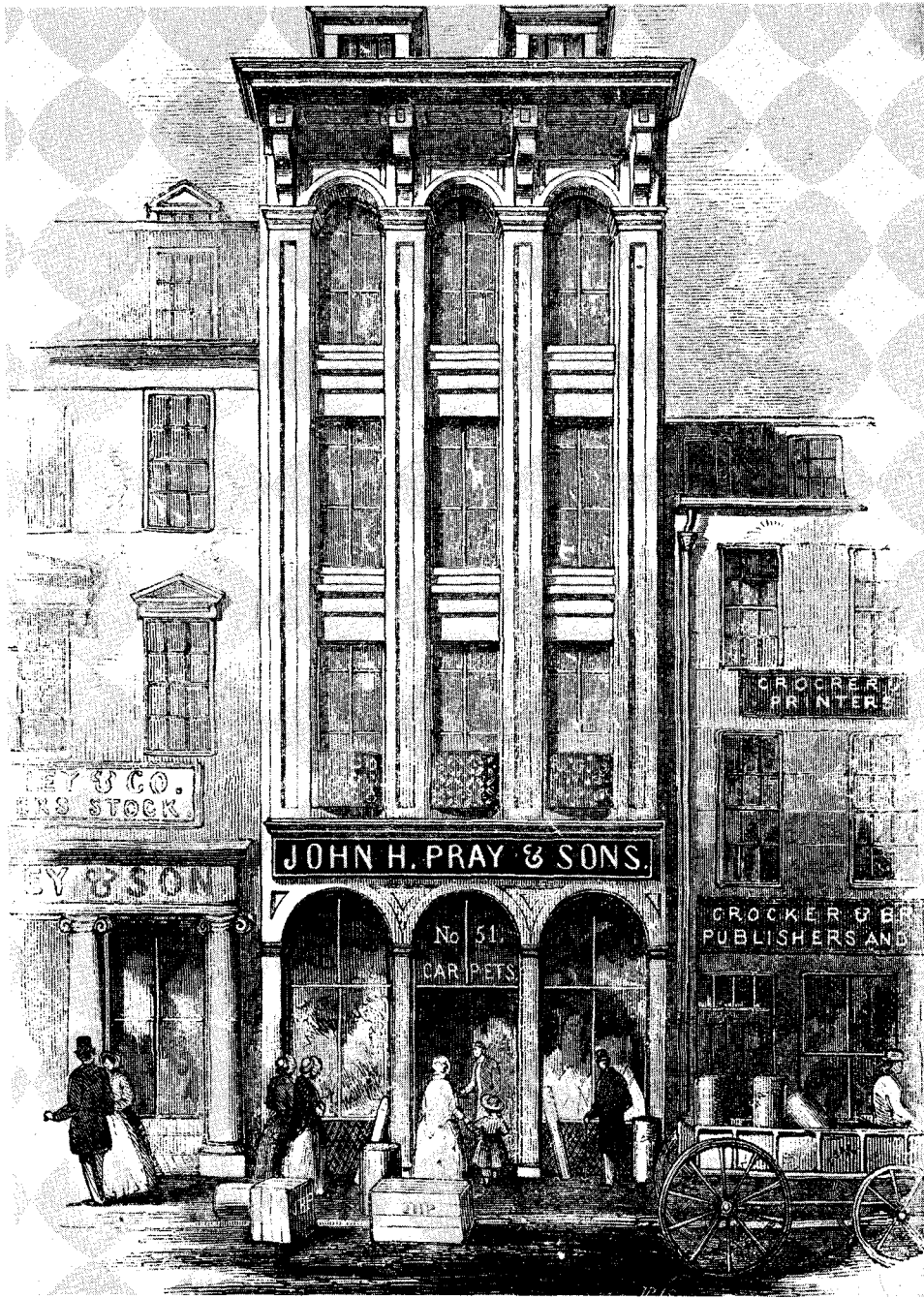


Figure 7: Exterior of a Boston carpet store in the 1850s. By the mid-nineteenth century, retail store fronts were becoming more elegant and used display windows and building ornamentation more boldly. When several floors were needed for display or storage, the entire building could be integrated into the design of the ground-floor facade. The result was an explicitly commercial structure, literally from top to bottom. Compare the upper stories of this building with those of the buildings in Figures 5 and 6. Engraving appears in *Ballou's Pictorial Drawing-Room Companion*, December 1, 1855, p. 348, and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y.

LORD & TAYLOR,

101

Importers and Wholesale and Retail Dealers in
DRY GOODS.



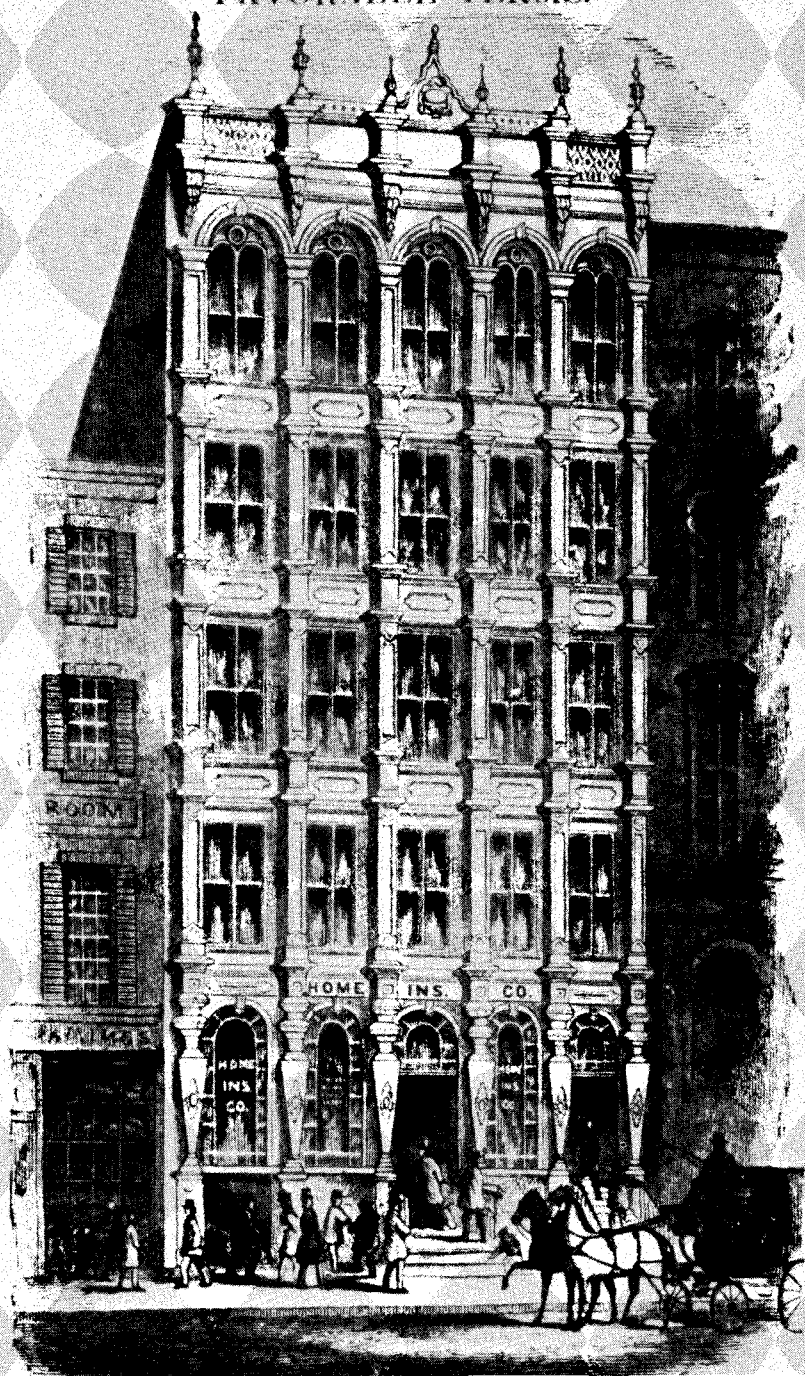
Nos. 255, 257, 259 & 261 Grand Street, cor. Chrystie, N. Y.

Figure 8: Retailing on a grand scale. Lord and Taylor was one of New York's largest and most fashionable dry goods stores. The size and elegance of structures such as this, and the goods displayed in their huge windows, helped make retailing a distinct and prominent activity within the commercial metropolis. Engraving reproduced courtesy of the Smithsonian Institution, Washington, D.C., National Museum of American History, Archives Center, Collection of Business Americana.

HOME INSURANCE COMPANY.

This Company insures against Loss and Damage by FIRE, on Buildings, Merchandise, Furniture, and all other kinds of Property, on FAVORABLE TERMS.

OFFICE: 112 AND 114 BROADWAY.



Cash Capital, \$600,000.

Surplus, over \$250,000.

Figure 9: An insurance company exterior of the late 1850s. The significance of office work in the major cities before the Civil War is suggested by this Broadway facade. Engraving appears in *Boyd's Pictorial Directory of Broadway* (New York, 1859) and is reproduced courtesy of Cornell University Libraries, Ithaca, N.Y.

consumption patterns of Americans in different economic circumstances. Yet there are those who have deduced, or at least sensed, a newly affluent middle-class way of life, still distinct from the ever more opulent life style of the very rich and increasingly distinct from the humble life style of manual workers who continued to struggle to meet the cost of basic necessities.⁶⁶ Warner has written:

In the years between 1827 and 1860 the new middle class enjoyed a number of important advances in everyday consumption. The bare floors, whitewashed walls, and scant furniture of middle-income eighteenth-century homes gave way to wool carpeting, wallpaper, and all manner of furnishings. The houses themselves became relatively cheaper and grew in size from three rooms to four-to-six rooms in row houses or flats in row houses. The children slept one to a bed, and indoor toilets became common in their homes. In contrast to the eighteenth century when the middle-income house generally included the shop, the husband now commonly worked in an office, store or shop outside the home, and the first-floor front room became a parlor instead of a work room. Mid-nineteenth-century families of the new middle class did not need to put their children to work in the family trade or shop; they could take full advantage of the new public grammar school education. Finally, they had grown prosperous enough to attend the increasing variety of offerings of commercial downtown entertainment.

Almost every item on this list of middle-class consumption gains lay beyond the reach of Philadelphia's artisan population.⁶⁷

This is an extremely suggestive statement, yet, as Warner himself has pointed out, it is really no more than "an impressionistic summary which attempts to reconcile the pessimistic accounts of labor histories with the more optimistic indices of economic historians."⁶⁸ Can this "impressionistic" idea of a newly distinctive middle-class way of life be substantiated (or dismissed) through systematic research? One method, noted above, would compare the typical incomes of various occupations with the costs of necessities and other consumer goods. Several such studies, focused entirely on the manual work force, have been completed, and these suggest that few married manual workers of the nineteenth century earned more than a subsistence income. To gain a margin of comfort, even the best-paid workers often relied on the small additional incomes contributed by working resident children.⁶⁹ This

Work, Space, Family, and Group Experience in the Nineteenth Century; Essays Toward an Interdisciplinary History of the City (New York, 1981), 240–76; Dorothy S. Brady, "Consumption and the Style of Life," in Lance E. Davis *et al.*, eds., *American Economic Growth: An Economist's History of the United States* (New York, 1972), 61–89; and Laurie, *Working People of Philadelphia*, 12. Edgar W. Martin attempted to define general consumption patterns on the eve of the Civil War but was unable to make many useful distinctions between the levels or patterns of consumption of different income groups; Martin, *The Standard of Living in 1860* (Chicago, 1942).

⁶⁶ There is a disjuncture between studies of the workingmen's level of living, cited above, and various contemporary descriptive accounts, written mainly by English working-class immigrants, that emphasize the prosperity of American workers. This probably can be accounted for by the inferior material condition of British workers and by the large number of single young men in the American labor force—men whose low pay still permitted the purchase of good food and clothing for themselves. See [James Dawson Burn], *Three Years Among the Working-Classes in the United States During the War* (London, 1865); and *The British Mechanic's and Labourer's Handbook and True Guide to the United States* (London, 1840). For an excellent discussion of young unmarried workers, see Richard Briggs Stott, "Workers in the Metropolis: New York City, 1820–1860" (Ph.D. dissertation, Cornell University, 1983).

⁶⁷ Warner, *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia, 1968), 66. Also see Spann, *New Metropolis*, 118–20. For contemporary views, see Isaac Ferris, "Men of Business: Their Home Responsibilities," in Alexander, *The Man of Business*, 24–25; and Burn, *Three Years Among the Working-Classes*, 81.

⁶⁸ Warner, *Private City*, 66 n.

⁶⁹ See, especially, Haines, "Poverty, Economic Stress, and the Family," 258–60. For a fascinating firsthand

implies that the new consumer goods that filled the advertising pages of nineteenth-century newspapers were in fact purchased mainly by Warner's better-rewarded "new middle class" of "downtown businessmen, downtown retailers, [and] owners and superintendents of manufacturing establishments."⁷⁰ Reasonable as this may seem, it still leaves much to be inferred, not only about levels but also about patterns of consumption among different economic strata and occupational groups. What specific consumption choices did members of these different strata or groups typically make? What kinds of goods can be associated with particular levels of wealth or income or with different kinds of occupations? Can we find a way of answering these questions in order to answer the further question of whether consumption, as a more or less deliberate form of human behavior, helped give shape to an emerging middle class?

The only historical documents that record the personal possessions of large numbers of people are estate inventories. Recognized in recent years by historians of the seventeenth and eighteenth centuries as an extremely useful source for the examination of early modern levels of wealth and consumption patterns, these inventories have been touched hardly at all by historians of the nineteenth century.⁷¹ Although fewer detailed inventories survive from the nineteenth century than from earlier periods, sufficient numbers do exist, and these may be combined with other sources, such as census enumerations, city directories, and credit reports, to provide detailed information about the personal belongings (at the time of death) of individuals whose occupations, total wealth, ages, household statuses, and other relevant characteristics can be specified with some precision and who can be placed fairly accurately within the classes or strata of the total local population at particular moments preceding their deaths.⁷² Many of the inventories, moreover, identify the rooms as well as the artifacts of the decedent's household, thereby permitting a fairly accurate physical reconstruction of just the sort that can confirm

account of working-class consumption just after the end of the nineteenth century, see Margaret F. Byington, *Homestead: The Households of a Mill Town* (New York, 1910).

⁷⁰ Warner, *Private City*, 65 n.

⁷¹ For studies of pre-nineteenth-century consumption patterns of specific classes or economic strata, see Carole Shammas, "The Domestic Environment in Early Modern England and America," *Journal of Social History*, 14 (1980): 3–24; Lois Green Carr and Lorena S. Walsh, "Changing Life Styles in Colonial St. Mary's County," *Working Papers from the Regional Economic History Research Center*, 1 (1978): 73–118; and Alice Hanson Jones, *Wealth of a Nation To Be: The American Colonies on the Eve of the Revolution* (New York, 1980), 195–257, "Wealth Estimates for the American Middle Colonies, 1774," *Economic Development and Cultural Change*, 18 (1970): esp. 131–32. For his discussion of middling life styles, see Jackson Turner Main, *The Social Structure of Revolutionary America* (Princeton, 1965), 132–33, 158–59. On the use of probate inventories as historical sources, see Alice Hanson Jones, *American Colonial Wealth: Documents and Methods*, 1 (New York, 1977): 3–24; and Gloria Main, "Probate Records as a Source for Early American History," *William and Mary Quarterly*, 3d ser., 32 (1975): 89–99. Also see the essays by Anton Schuurman and Rachel P. Garrard in Ad Van der Woude and Anton Schuurman, eds., *Probate Inventories: A New Source for the Historical Study of Wealth, Material Culture, and Agricultural Development* (Wageningen, 1980), 19–31, 55–82.

⁷² Linking probate inventories to the manuscript schedules of the census immediately preceding the death of the decedent can provide crucial information that may not be available in other sources. For example, knowing whether the decedent was a head of household or a boarder is essential for any analysis of household furnishings by wealth or occupational group. The property listings (on the 1860 and 1870 censuses) permit a more accurate and less biased comparison of the wealth of the decedent with the wealth distribution of the population as a whole. Age, race, occupation, and birthplace are specified on all censuses after 1850. A census index now in preparation should make the task of linking external records to censuses much less onerous.

or disprove Warner's impressionistic description of the emerging middle-class domestic interior.

The parlor of Joseph P. Engles, a Philadelphia publishing agent who died in 1861, contained two sofas, thirteen chairs, three card tables, a "fancy table," a piano and stool, a looking glass, and five yards of ingrain carpet. By contrast, the parlor of wheelwright Samuel Heritage contained only a maple rocking chair, a walnut work stand, a looking glass, and a carpet, whereas the "front room" of barkeeper Henry Creighton contained seven cane-seat chairs, two "common chairs," a table, a stand, and a carpet, all very inexpensive. Other rooms of Engles's house were better and more fully furnished and carpeted than were Heritage's and Creighton's, and the latter combined his family's living quarters with his ground floor barroom. The agent's house, in short, conformed well to Warner's description of a middle-class interior of the mid-nineteenth century, while the wheelwright's and the barkeeper's did not.⁷³ But not even a very large accumulation of cases such as these will suffice to reach a firm conclusion without supplementary evidence that locates each individual more accurately within the social structure. Census schedules are the critical source, but even city directories can be useful, as in the case of Simeon Tobey, who called himself a "mariner" in his will but whose inventory shows a large home stuffed with expensive furniture and silver.⁷⁴ The directories identify Tobey as a sea captain who, for many years after he left the sea, served as president of the Pennsylvania Insurance Company.

One aspect of consumption that has caught the attention of historians interested in middle-class life is the purchase or rental of new homes in distinctly middle-class suburban neighborhoods, a phenomenon that several scholars have associated with the more general spatial deconcentration and reorganization of major cities during the second half of the nineteenth century.⁷⁵ Thus, residential location and the evolution of visibly middle-class residential milieux emerge as significant themes in the study of middle-class formation. For not only does location in a recognized middle-class neighborhood become a badge of middle-class respectability but daily interactions within a homogeneous social milieu also serve to build up and reinforce both the substance and the boundaries of the middle class as a distinctive social group. Do we, then, find in the "streetcar suburbs" of the late nineteenth century the culmination and physical embodiment of middle-class formation? The answer is not yet clear. Indeed, the most recent and technically advanced research, based on

⁷³ Register of Wills, Philadelphia County, 1861, will nos. 157, 165, 59.

⁷⁴ *Ibid.*, 1861, will no. 70.

⁷⁵ The classic study is Sam Bass Warner, Jr., *Streetcar Suburbs: The Process of Growth in Boston, 1870-1900* (Cambridge, Mass., 1962). Also see Roger D. Simon, "The City-Building Process: Housing and Services in New Milwaukee Neighborhoods, 1880-1910," *Transactions of the American Philosophical Society*, 68 (1978): 3-64; Roger Miller and Joseph Siry, "The Emerging Suburb: West Philadelphia, 1850-1880," *Pennsylvania History*, 47 (1980): 99-145; Kenneth T. Jackson, "Urban Deconcentration in the Nineteenth Century: A Statistical Inquiry," in Leo F. Schnore, ed., *The New Urban History: Quantitative Explorations by American Historians* (Princeton, 1975), 110-42; Kathleen Neils Conzen, "Patterns of Residence in Early Milwaukee," in Schnore, *New Urban History*, 145-83; Theodore Hershberg et al., "The 'Journey-to-Work': An Empirical Investigation of Work, Residence, and Transportation; Philadelphia, 1850 and 1880," in Hershberg, *Philadelphia*, 128-73; Alan N. Burstein, "Immigrants and Residential Mobility: The Irish and Germans in Philadelphia, 1850-1880," in Hershberg, *Philadelphia*, 174-203; and William W. Cutler III and Howard Gillette, Jr., *The Divided Metropolis: Social and Spatial Dimensions of Philadelphia, 1800-1975* (Westport, Conn., 1980).

the detailed records of the Philadelphia Social History Project, suggests that urban deconcentration after 1850 did not lead to more homogeneous neighborhoods in the city as a whole and that even by 1880 only a small proportion (less than 20 percent) of the white-collar work force of Philadelphia lived in relatively homogeneous neighborhoods in which white-collar workers were the dominant group. On the other hand, white-collar workers formed the only occupational group that experienced an increase in the proportion of the group living in homogeneous neighborhoods (their 1850 proportion was less than 10 percent), while less heavily populated neighborhoods near the periphery of the city (presumably these were the upper-class and middle-class suburbs) were decidedly more homogeneous than were the more heavily populated neighborhoods near either the periphery or the center.⁷⁶ Moreover, there are important limitations in the data used in this study that probably cause residential homogeneity to be understated. Homogeneity is defined entirely in occupational terms, and the occupational categories distinguish white-collar proprietors and employees from *all* artisans, including those proprietary artisans whom we earlier described as essentially white-collar managers, contractors, and manufacturing retailers. If these proprietary artisans were moved to the white-collar category (their average wealth was significantly higher than other artisans and similar to those classified as "low white collar" in the Philadelphia data),⁷⁷ a more homogeneous residential pattern might well emerge. More important, the data do not distinguish between street-front and alley-front locations, which in Philadelphia were extremely important forms of residential segregation by income group from the colonial period right through the nineteenth century.⁷⁸ As Roger Miller and Joseph Siry recently noted, "A typical building scheme of the 1840s and 1850s was the simultaneous construction of a magnificent townhouse on one of the city's primary east-west streets and provision of worker rows in the mews, fronting on alleys cut through the interior of the block."⁷⁹ Both the wealthy owner of the townhouse and the alley-dwelling workers occupied the same block (and the same areal grid-unit in the Philadelphia Social History Project data), but they inhabited separate social worlds, expressed in a pattern of spatial segregation too fine to be captured even by an areal analysis whose units are as small as a city block. Significantly, Miller and Siry found this same phenomenon in the new suburb of West Philadelphia, the distinctly middle-class character of which

⁷⁶ Janet Rothenberg Pack, "Urban Spatial Transformation: Philadelphia, 1850–1880: Heterogeneity to Homogeneity?" *Social Science History*, 8 (1984): 425–54. Pack concluded that low density, rather than class-related residential selection, is a more important explanation of the homogeneity of peripheral neighborhoods. But Pack did not appear to consider that the more densely settled peripheral neighborhoods were not upper-class or middle-class suburbs but industrial districts, such as Manayunk, a cluster of mills and workers' houses on the Schuylkill River. Distinguishing between these two types of peripheral land use would no doubt increase the importance of class-related selection as an explanation of residential homogeneity.

⁷⁷ For a discussion of the Philadelphia Social History Project's vertical classification scheme, see Theodore Hershberg and Robert Dockhorn, "Occupational Classification," *Historical Methods Newsletter*, 9 (1976): 67–68, and esp. Table 3, p. 71.

⁷⁸ William S. Hastings, "Philadelphia Microcosm," *Pennsylvania Magazine of History and Biography*, 91 (1967): 167–80; Elizabeth Gray Kogen Spera, "Building for Business: The Impact of Commerce on the City Plan and Architecture of the City of Philadelphia, 1750–1800" (Ph.D. dissertation, University of Pennsylvania, 1980), 87; and Burstein, "Immigrants and Residential Mobility," 183.

⁷⁹ Miller and Siry, "Emerging Suburb," 143.

is probably further obscured by the temporary presence of large numbers of construction workers during the period of rapid development.⁸⁰ For more permanent inhabitants, the street-fronting house in West Philadelphia may well have been both the ultimate middle-class consumer good and the passport to a more homogeneously middle-class social world.

Of course, social worlds are also shaped by formal institutions and informal networks that draw people together according to criteria that may have little to do with physical proximity. Since Tocqueville, observers and historians of American society have stressed the propensity of Americans to join together in voluntary associations of all sorts and for all purposes, from reform societies whose goals were to cleanse the society from sundry particular evils, to lyceums and music societies intended to provide elevating recreation and instruction, to lodges and clubs formed for the purpose of enhancing and organizing sociability. Although a number of scholars have inquired into the social characteristics of the joiners of various organizations, few have asked whether associations may have contributed, unintentionally or by design, to class formation. One recent exception is John Gilkeson, Jr., who proposed, in a dissertation completed in 1981, a direct connection between the emergence of reform organizations (in particular temperance and antislavery societies) and the crystallization of middle-class consciousness in the antebellum era. The specific issues of drink and slavery underscored to reformers their own intermediate social and economic circumstances, for resistance to both temperance and abolitionism seemed to come from the fashionable and powerful rich and from the dissipated and fearful poor. Furthermore, in pursuit of their specific goals and against specific opposition from above and below, the temperance and antislavery societies "propagated the [broader] values—industry, thrift, sobriety, and civic duty—around which the American middle class coalesced."⁸¹ "From this perspective," wrote Paul Boyer, "the decision to participate in an urban moral-reform society might reflect less the wish to control others than an impulse toward self-definition, a need to avow publicly one's own class aspirations."⁸² Perhaps, too, it expressed a wish to learn how to *behave* according to rules of middle-class respectability,⁸³ a point that may apply as well to less reform-minded associations, such as debating societies, militia companies, fraternal lodges, and others dedicated at least in part to the organization of sociability among members of and aspirants to an emerging middle class. Not merely sociability but also socialization may have been involved in the innumerable meetings, balls, soirees, debates, excursions, and other affairs sponsored by even the most low-keyed of voluntary associations.

Propositions such as these require an understanding of both the ideology of

⁸⁰ *Ibid.*, 99–145.

⁸¹ Gilkeson, "City of Joiners," 47. For other recent analyses of the temperance movement from a similar point of view (though not as clearly linked to the concept of middle-class formation), see Ian R. Tyrrell, *Sobering Up: From Temperance to Prohibition in Antebellum America, 1800–1860* (Westport, Conn., 1979); and Robert L. Hampel, *Temperance and Prohibition in Massachusetts, 1813–1852* (Ann Arbor, 1982).

⁸² Boyer, *Urban Masses*, 61. Also see Ryan, *Cradle of the Middle Class*, 13, 135. For a similar point made with reference to reform-minded Whigs, see Howe, *Political Culture*, 29, 36–37, 300.

⁸³ Gilkeson has called temperance "a program for middle-class respectability"; "City of Joiners," 13.

reform and the motives, practices, and patterns of organizational membership of local associations. My own study of Kingston, New York, and Michael Katz's first volume on Hamilton, Ontario, suggest that in smaller cities participation in voluntary organizations was general within but limited to the entrepreneurial community. Thus, in Kingston, as many as four of every ten businessmen and professionals, but only four of every hundred of the unskilled and semiskilled work force, could be identified on the rolls of the town's voluntary organizations. Most of the entrepreneurial joiners, moreover, were active in several organizations, while only thirteen workers, fewer than 1 percent of the total, were more than minimally active in local organizational life.⁸⁴ Those who have studied the organizations of larger cities, however, have described more complicated patterns, with some types of organizations limited to the very wealthy and others, mainly based in specific neighborhoods, limited to the working class.⁸⁵ Boyer also distinguished between the wealthy and prominent leaders of big-city reform societies and their "urban-morality foot soldiers" who "were often first-generation urbanites active in evangelical churches and holding what would later be called white-collar jobs—clerks, accountants, bank tellers—on the lower rungs of the business or professional ladders."⁸⁶ Were most organizations in the larger cities so clearly linked to class in their memberships and internal arrangements? Was there, in addition to the atheneums, reform societies, and workingmen's fire companies, a tier of organizations open only to middling folk? If so, was this primarily a neighborhood phenomenon, expressive mainly of the spatial segregation of classes within the city, or did these organizations span the neighborhoods to provide a common ground for social life among the middle classes? These sorts of patterns should be understood before records of specific organizations can reveal whether exclusivity and class definition were the products (or more boldly, the purposes) of associational life.

Finally, there is the question of how family organization and "strategy" may have contributed to middle-class formation. As I noted earlier, one of the most forceful statements of the hypothesis of nineteenth-century middle-class formation, Mary Ryan's *Cradle of the Middle Class*, places family strategy at the very center of the process. "Early in the nineteenth century," wrote Ryan, "the American middle class molded its distinctive identity around domestic values and family practices."⁸⁷ By mid-century new attitudes toward child nurturance and an enhanced role for the mother in socializing and cultivating the sensibilities of the child led to a set of

⁸⁴ Stuart M. Blumin, *The Urban Threshold: Growth and Change in a Nineteenth-Century American Community* (Chicago, 1976), 174–76, 227–29. Some 22 percent of Kingston's clerks and artisans were identified as organizational participants. Unfortunately, I was unable to divide the artisans into the categories I discuss in this essay. Also see Katz, *People of Hamilton*, 178–83.

⁸⁵ See Pessen, *Riches, Class, and Power before the Civil War*; Jaher, *Urban Establishment*; Story, *Forging of an Aristocracy*; Lee L. Schreiber, "Bluebloods and Local Societies: A Philadelphia Microcosm," *Pennsylvania History*, 48 (1981): 251–66; Bruce Laurie, "Fire Companies and Gangs in Southwark: The 1840s," in Allen F. Davis and Mark H. Haller, eds., *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790–1940* (Philadelphia, 1973), 71–87; and Warner, *Private City*, 62. Hirsch found only a small amount of mixing of classes in Newark's voluntary organizations before 1860; *Roots of the American Working Class*, 100–02. Faler found virtually none in antebellum Lynn; *Mechanics and Manufacturers*, 189–221.

⁸⁶ Boyer, *Urban Masses*, 15.

⁸⁷ Ryan, *Cradle of the Middle Class*, 15.

specific strategies for securing each child's and the whole family's foothold in the emerging middle class. First, "prescient native-born couples" began to limit the size of their families, "thereby concentrating scarce financial and emotional resources on the care and education of fewer children." Second, these same couples "initiated methods of socialization designed to inculcate values and traits of character deemed essential to middle-class achievement and respectability," values and traits not of the aggressive entrepreneur but of the "cautious, prudent small-business man" (thereby helping to define "the upper as well as the lower boundaries of the middle class"). Third, children were kept within their parents' household for longer periods, prolonging parental surveillance and material support. Fourth, these resident children were given greater amounts of formal schooling, a crucial tactic intended to help them secure positions in the expanding white-collar work force. And finally, the young men who emerged from this middle-class cradle were encouraged to delay their marriages until they had secured their own middle-class positions, a tactic that also contributed to the perpetuation of the tactical sequence, in that it encouraged the reproduction of small families.⁸⁸

The implication of all this (and not merely the final tactic) is that the sequence was self-perpetuating—that once implemented, these strategies would succeed in gaining or securing each family's position in the middle class, and established middle-class families would be the ones most likely to pursue the same strategies in subsequent generations. Ryan did not deny that "prescient" working-class couples could adopt the same child-rearing tactics and thereby help push their offspring into the middle class, but the very essence of her argument is that the process was one that tended to separate and define classes, not to conflate or weaken them. Ryan has received some support here from historians of education who point out that children from working-class families attended school less frequently and for fewer years than did children from middle-class families,⁸⁹ and from economic historians who have emphasized the common practice in workingmen's households of putting even quite young children to work as a necessary source of additional income.⁹⁰ Thus, large numbers of workers either did not attempt to implement middle-class child-rearing strategies or found that their attempts broke down at the crucial stage where status-enhancing values were to be joined with status-enhancing skills.

Were the poorly schooled sons of workingmen thereby denied access to the white-collar jobs that conferred membership in the middle class? Clyde Griffen and

⁸⁸ *Ibid.*, 161, 184–85.

⁸⁹ Michael B. Katz, *The Irony of Early School Reform: Educational Innovation in Mid-Nineteenth-Century Massachusetts* (Cambridge, Mass., 1968), esp. 39, 90–93, 270–71; Selwyn Troen, "Popular Education in Nineteenth-Century St. Louis," *History of Education Quarterly*, 13 (1973): 23–40; Stanley K. Schultz, *The Culture Factory: Boston Public Schools, 1789–1860* (New York, 1973), 278–309; David B. Tyack, *The One Best System: A History of American Urban Education* (Cambridge, Mass., 1974), 67; Carl F. Kaestle, *The Evolution of an Urban School System: New York City, 1750–1850* (Cambridge, Mass., 1973), 88–100; and Carl F. Kaestle and Maris A. Vinovskis, *Education and Social Change in Nineteenth-Century Massachusetts* (Cambridge, Mass., 1980), 9–45, and esp. 87–90.

⁹⁰ Michael Haines found a very high proportion of working children (69.3 percent of resident children aged 11 and older) within the households of white industrial workers in late nineteenth-century Philadelphia; "Poverty, Economic Stress, and the Family," 260. Also see Claudia Goldin, "Family Strategies in Late Nineteenth-Century Philadelphia," *Working Papers from the Regional Economic History Research Center*, 2 (1979): 60–106.

Sally Griffen's careful study of occupational mobility in nineteenth-century Poughkeepsie (where very few sons of manual workers went to high school) suggests that many of them were. In Poughkeepsie, at least, the sons of white-collar workers were six times as likely as the sons of blue-collar workers to start their careers in white-collar jobs. And those sons of white-collar workers who did not move at once into white-collar work—who seemingly confounded their parents' strategy by slipping down into the manual working class—were significantly more likely than the blue-collar sons of blue-collar fathers to move into white-collar jobs during the course of their careers.⁹¹ This does not, of course, prove that child rearing or other specifically familial strategies constituted the principal mechanism of "middle-class reproduction." But it certainly lends plausibility to Ryan's argument and suggests the value of continuing research into the differences between, and differing consequences of, middle-class and working-class familial practices.

IT SHOULD BE EMPHASIZED that the five substantive areas of social experience that I have defined and described above, and any others that may be added to the list, must not be analyzed in isolation from one another. On the contrary, the final and most critical question to be raised in any test of the hypothesis of middle-class formation is whether the various dimensions of experience converge in such a way that they constitute a distinct way of life for a distinct subgroup of the population. Put another way, the boundaries that were established between upper, intermediate, and lower strata by changes in work, consumption, the spatial structure of urban neighborhoods, formal and informal group life, the organization and child-rearing strategies of families, and any other definable areas of relevant experience must coincide to such a degree that each type of experience reinforces the distinctiveness of each stratum. This convergence or coincidence of class-relevant experiential boundaries is what Giddens means by "structuration" and represents the point at which one can begin to speak of classes rather than strata. Did these boundaries coincide in nineteenth-century America? Do we speak of the same people when we refer to white-collar entrepreneurs and workers, to the owners of comfortably furnished fifteen-foot parlors, to the residents of attractive (but not "fashionable") city or suburban neighborhoods, to the members of temperance societies and lyceums, and to parents who taught their children to be thrifty and industrious and who sent them to high school in preparation for careers in business or business employment? Much of the above discussion suggests an affirmative answer. There appears to have been a sufficient convergence of personal and social experience to give credence to the idea of an emerging middle-class way of life and to give at least preliminary support (even in the absence of significant evidence of middle-class consciousness) to the hypothesis of middle-class formation.⁹²

Subsequent research will, I am sure, confirm some of these convergences, discard

⁹¹ Griffen and Griffen, *Natives and Newcomers*, 66–67, 80–83.

⁹² Paul Johnson has written, "It was not only the need for clearheaded calculation at work but the new ethos of bourgeois family life that drove businessmen away from the bottle"; *Shopkeeper's Millennium*, 57. Here is a statement that associates three areas of class-forming experience—a calculating ("headworking") businessman, responding to a new, specifically bourgeois family ethos, adopts a new mode of behavior, spurred on most likely by his association with a local temperance society.

others, and suggest modifications and variations in the hypothesis itself. For example, differences in the distinctiveness of the boundary between local middle and upper classes may impart different meanings to the concept of middle class (and hence to middle-class formation) in larger and smaller cities. Several of the more forceful statements of middle-class formation are to be found in studies of smaller cities such as Utica, early Rochester, and Providence. Would the same statements emerge, and with equal force, from equivalent studies of New York or Boston? Surely we can expect to find different patterns in different local (and regional) settings, and whether the hypothesis of middle-class formation can be modified to incorporate these variations without losing all meaning remains a real question. It may require modifications, too, in order to recognize the existence of subdivisions of the middle class, or of two or more distinct intermediate classes, or of greater degrees of coalescence at some levels of the middle class than at others. What is required, in sum, is an approach to middle-class formation that is alert to both convergence and variation in the historical experience of middling folk. On the foundation of such an approach, historians can examine further the cultural and ideological dimensions of middle-class formation, including the extent to which specific values and beliefs were articulated by and on behalf of a definable and self-conscious middle class. And they can relate all of these patterns of social experience and thought to the formation of classes in general and to the larger questions of whether, and in what ways, classes were significant in the development of modern American society.

Capitalism and the Origins of the Humanitarian Sensibility, Part 1

THOMAS L. HASKELL

AN UNPRECEDENTED WAVE OF HUMANITARIAN reform sentiment swept through the societies of Western Europe, England, and North America in the hundred years following 1750. Among the movements spawned by this new sensibility, the most spectacular was that to abolish slavery. Although its morality was often questioned before 1750, slavery was routinely defended and hardly ever condemned outright, even by the most scrupulous moralists. About the time that slavery was being transformed from a problematical but readily defensible institution into a self-evidently evil and abominable one, new attitudes began to appear on how to deter criminals, relieve the poor, cure the insane, school the young, and deal with primitive peoples.¹ The resulting reforms were, by almost any reasonable standard, an improvement over old practices that were often barbarous. Even so, twentieth-century historians have not been satisfied to attribute those reforms either to an advance in man's moral sense or, simply, to a random outburst of altruism. In explaining the new humanitarianism, historians have repeatedly pointed to changes in what Marxists generally call the economic base or substructure of society, that is, the growth of capitalism and beginnings of industrialization. Tracing links between humanitarianism and capitalism has been a major preoccupation of historians, and the enterprise has succeeded, I believe, in greatly extending our understanding of the new sensibility. We know now that the reformers were motivated by far more than an unselfish desire to help the downtrodden, and we

So many people have given me advice about an earlier and briefer version of this essay that I can scarcely call it my own, except insofar as it errs or offends. My principal debt is to David Brion Davis, whose extraordinarily generous and thoughtful correspondence saved me from many errors of fact, taste, and judgment. The essay was first presented to the Social Science Seminar of the Institute for Advanced Study, Princeton, N.J., in April 1979. Since then I have also benefited from discussions with members of the Social Science Seminar at Rice University and the Department of History and Philosophy, Carnegie-Mellon University. For especially thorough and helpful comments, I am grateful to Seymour Drescher, Stanley Engerman, Ira Gruber, Albert Hirschman, Jay Hook, Jackson Lears, Elizabeth Long, George Marcus, Lewis Perry, Andrew Scull, Quentin Skinner, Peter Stearns, Richard Teichgraber, Larry Temkin, Mark Warren, Roger Wertheimer, Morton White, and Bertram Wyatt-Brown. Martin Wiener's encouragement and advice have been especially valuable. The work was made possible by leaves funded by the Institute for Advanced Study, the Rockefeller Foundation, and the Dean of Humanities, Rice University. It is part of a larger study in the history of moral responsibility in Anglo-American culture after 1750.

¹ David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, 1966).

see more clearly now why their reforms went no farther and took the particular form they did.² Historians are never again likely to believe, as did W. E. H. Lecky in 1876, that Britain's campaign against slavery was "among the three or four perfectly virtuous acts recorded in the history of nations."³

But these advances in understanding have been achieved only at the expense of a growing ambivalence as we try to acknowledge two things at once: that humanitarian reform not only took courage and brought commendable changes but also served the interests of the reformers and was part of that vast bourgeois project that Max Weber called rationalization. This ambivalence reached painful heights in Michel Foucault's *Discipline and Punish*, in which he questioned whether there really was a new humanitarian sensibility and argued that, though a new sensitivity to suffering did exist, its aim in prison reform was not humane. Its real aim, Foucault concluded, was "not to punish less, but to punish better; to punish with an attenuated severity perhaps, but in order to punish with more universality and necessity; to insert the power to punish more deeply into the social body."⁴ Foucault's position contains much truth, yet in contemplating it we must not lose sight of another truth, namely, that to put a thief in jail is more humane than to burn him, hang him, maim him, or dismember him.

The inadequacy of prevailing modes of explanation tempts scholars to migrate toward two extremes: either to abandon the very idea of humanitarianism lest it veil the play of domination or to reassert the classical liberal view that humanitarian ideas belong to a transcendent realm of moral choice, which no inquiry into social or economic circumstances can hope to illuminate. The latter strategy, essentially one of compartmentalization, finds considerable support in Roger Anstey's sophisticated effort to refurbish the traditional image of British abolitionists as moral giants. Even more decisive encouragement comes from Seymour Drescher's *Econocide*, which seeks to show, contrary to the thesis of Eric Williams, that slavery was a profitable and important part of the British economy and that the decision to abolish it ran directly counter to Britain's economic interest.⁵

The present historiographical dilemma has been aptly described by Howard Temperley. To argue that "abolition had nothing to do with economics except insofar as economic interest was a factor to be overcome," he observed, leads to conclusions that are, "to put it mildly, a little odd."

Here we have a system—a highly successful system—of large-scale capitalist agriculture, mass

² For the touchstone of what has become a large body of literature, see Eric Williams, *Capitalism and Slavery* (Chapel Hill, 1944). Also see C. L. R. James, *The Black Jacobins: Toussaint L'Ouverture and the San Domingo Revolution* (New York, 1963); Roger Anstey, "Capitalism and Slavery: A Critique," *Economic History Review*, 2d ser., 21 (1968): 307–20; Eugene Genovese, "Materialism and Idealism in the History of Negro Slavery in the Americas," in Laura Foner and Eugene Genovese, eds., *Slavery in the New World: A Reader in Comparative History* (Englewood Cliffs, N.J., 1969), 238–55; Christine Bolt and Seymour Drescher, eds., *Anti-Slavery, Religion, and Reform: Essays in Memory of Roger Anstey* (Hamden, Conn., 1980); and James Walvin, ed., *Slavery and British Society, 1776–1846* (Baton Rouge, La., 1982). Additional major contributions to the controversy by Anstey, Davis, Drescher, and Temperley are cited below.

³ Lecky, as quoted in David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, 1975), 353.

⁴ Foucault, *Discipline and Punish: The Birth of the Prison*, trans. Alan Sheridan (New York, 1977), 82.

⁵ Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (Atlantic Highlands, N.J., 1975); and Drescher, *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, 1977).

producing raw materials for sale in distant markets, growing up at a time when most production was still small-scale and designed to meet the needs of local consumers. But precisely at a time when capitalist ideas were in the ascendant, and large-scale production of all kinds of goods was beginning, we find this system being dismantled. How could this happen unless "capitalism" had something to do with it? If our reasoning leads to the conclusion that "capitalism" had nothing to do with it, the chances are that there is something wrong with our reasoning.⁶

I believe that a real change in sensibility occurred, and that it was associated with the rise of capitalism. The way out of the current historiographical impasse is to find a way to establish the connection without reducing humane values and acts to epiphenomena in the process. To do this we must begin by reexamining the ways in which substructural developments like the rise of capitalism might have influenced superstructural developments like humanitarianism. There is more than one way in which these phenomena might be linked, and the purpose of this essay is to bring into focus a kind of linkage that historians have sometimes tacitly assumed but never explored in a deliberate and systematic way.

Today the most popular way to formulate the linkage between capitalism and the humanitarian sensibility goes under the banner of "social control" or "class hegemony." Reduced to its basic outlines, this scheme of explanation rests on two assumptions. The first concerns what we really have in mind when we use the umbrella phrase "rise of capitalism," which, after all, covers a large cluster of quite diverse concrete developments, any one of which we might think more important than others. When social control theorists use this phrase, they usually mean only one of the elements hidden under the umbrella—the ascendancy of a new, entrepreneurial class, the bourgeoisie. This new class is understood to have distinctive interests deriving from its control over the society's predominant means of production. Those interests are understood to be such that the class will favor any measure that ensures the docility of the less advantaged sectors of the population, that enhances the discipline and productivity of the work force on which the economy depends, that strengthens its own morale or weakens that of other groups, or that contributes in any other way to the maintenance of its own supremacy.

The second assumption basic to the social control interpretation flows naturally from the first: class interest is the medium—and presumably the *only* important medium—through which substructural change influences developments in the superstructure. Given these assumptions and the bourgeois origins of almost all humanitarian reformers of the late eighteenth and early nineteenth centuries, the

⁶ Temperley, "Capitalism, Slavery, and Ideology," *Past and Present*, 75 (1977): 105. I wrote the first draft of this essay before reading Temperley's article, but his aim in many ways parallels my own. And I certainly share Temperley's view that the question to answer is "how could a philosophy which extolled the pursuit of individual self-interest have contributed, in the absence of any expectation of economic gain, to the achievement of so praiseworthy an object as the abolition of slavery." *Ibid.*, 117. I am not convinced, however, that a full answer to this question can be gotten by examining capitalism as an ideology. Also see Temperley, "Anti-Slavery as a Form of Cultural Imperialism," in Bolt and Drescher, *Anti-Slavery, Religion, and Reform*, 335–50. In his later article, Temperley stressed the convergence of nationalism, an accelerating pace of economic growth, and notions of naturally or divinely ordained progress, factors that mesh closely with the somewhat more abstract argument to be presented here.

strategy of explanation becomes obvious: the way for the social control historian to explain humanitarianism is to show how supposedly disinterested reforms actually functioned to advance bourgeois interests. To state the explanatory schema so baldly makes it sound simpler and more vulnerable than it really is. The social control thesis, like any other, is capable of sophisticated as well as crude applications, as the following discussion of a very refined application of the thesis should make clear. But however indirectly and subtly reform may be said to have served class interest, the historian employing the social control schema is strongly predisposed to look to class interest alone for the connecting link between capitalism and humanitarianism, base and superstructure.

The alternative interpretation that I shall present rejects both assumptions of the social control thesis. Without questioning the great importance of self-interest and class interest in human affairs, and while fully recognizing that interests exert an important influence on belief through what Weber called "elective affinity," I shall argue that in this particular inquiry the concept of class interest has obscured almost as much as it has revealed.⁷ Stated plainly, my thesis is this: Whatever influence the rise of capitalism may have had generally on ideas and values through the medium of class interest, it had a more telling influence on the origins of humanitarianism through changes the market wrought in *perception* or *cognitive style*. And it was primarily a change in cognitive style—specifically a change in the perception of causal connection and consequently a shift in the conventions of moral responsibility—that underlay the new constellation of attitudes and activities that we call humanitarianism. What altered cognitive style in a "humanitarian" direction was not in the first instance the ascendancy of a new class, or the assertion by that class of a new configuration of interests, but rather the expansion of the market, the intensification of market discipline, and the penetration of that discipline into spheres of life previously untouched by it. To explain humanitarianism, then, what matters in the capitalist substructure is not a new class so much as the market, and what links the capitalist market to a new sensibility is not class interest so much as the power of market discipline to inculcate altered perceptions of causation in human affairs.

This approach has certain advantages. Instead of prompting the historian to unmask the interestedness of ostensibly disinterested reforms, the explanatory approach advocated here would lead the historian to demonstrate the "naturalness" of these reforms, given the historical development of certain cognitive structures that were formed in the crucible of market transactions. Because these cognitive structures underlay *both* the reformers' novel sense of responsibility for others and their definition of their own interests, there is indeed a certain congruence between the reforms they carried out and the needs of their class. The social control

⁷ Although Weber's treatment of the relationship between ideas and interest in *The Protestant Ethic and the Spirit of Capitalism* seems to me the best model we have, Weber used the concept of elective affinities in very diverse ways. See Weber, *The Protestant Ethic and the Spirit of Capitalism*, trans. Talcott Parsons (New York, 1958, 1976); and Richard Herbert Howe, "Max Weber's Elective Affinities: Sociology within the Bounds of Pure Reason," *American Journal of Sociology*, 84 (1978): 366–85. Also see Weber, "Religious Ethics and the World," in Guenther Roth and Claus Wittich, eds., *Economy and Society: An Outline of Interpretive Sociology*, trans. Ephraim Fischhoff *et al.*, 2 (New York, 1968): 577.

argument errs not in stressing the existence of this congruence but in the account given of its origins. The approach recommended here does not aim to turn the social control argument on its head, retaining its opposition of ideas and interests while reversing their causal relationship; instead, the purpose is to overcome this dualism altogether by acknowledging that ideas and interests are interwoven at every level and in fact arise from the same source—a certain way of perceiving human relations fostered by the forms of life the market encouraged.

In another respect, however, the reader will note that my argument does reverse what is commonly thought to be the proper order of things. The pervasive, if diffuse, influence of the neo-Freudian tradition has prepared us to accept without much question the idea that feelings influence perception, that our emotional needs shape the way we see and experience the world around us. Although I do not doubt in the least that emotion has the power to influence perception, and often does, the present study shows, I believe, that the reverse can also be true. The rise of antislavery sentiment was, among other things, an upwelling of powerful feelings of sympathy, guilt, and anger, but these emotions would not have emerged when they did, taken the form they did, or produced the same results if they had not been called into being by a prior change in the perception of causal relations.

The order of argument is roughly as follows. In order to specify more exactly the dilemmas inherent in the social control interpretation, I will examine the most penetrating and sophisticated example of that approach—David Brion Davis's *The Problem of Slavery in the Age of Revolution*. My aim is to show that, although this book's sophistication has many sources, one is the tendency to play down class interest (even while finally embracing it) by stressing the concept of self-deception; that, in trying to avoid making class interest the exclusive or overpowering link between substructural and superstructural change, Davis naturally moved in the direction of cognitive style; and that, by going one step further in the same direction, some residual ambiguities in his analysis can be clarified. Once the need has been established, a new formulation can be attempted.

DAVIS MOVED SO FAR BEYOND the ordinary limitations of the social control thesis that one is tempted to credit him with having superseded it. Certainly much of the alternative approach that I wish to recommend is implicit in Davis's analysis. He never denied the authenticity of the reformers' good intentions and never claimed that their "actual" aim was to achieve social control. He was, however, content to depict the antislavery movement as peculiarly susceptible to efforts to convert it into a "vehicle for social control," and, even after all of his many qualifications are taken into account, class interest is the only link between base and superstructure that he specifically recognized.⁸ For Davis, "the key questions concern the relationship between antislavery and the social system as a whole."

Why did a seemingly liberal movement emerge and continue to win support from major government leaders in the period from 1790 to 1832, a period characterized by both political

⁸ Davis, *The Problem of Slavery in the Age of Revolution*, 379.

reaction and industrial revolution? How could such a movement be embraced by aristocratic statesmen and yet serve eventually as a vehicle for the triumphant middle class, who regarded West Indian emancipation as the confirmation of the Reform Bill of 1832, and who used antislavery rhetoric and strategy as models for their assault upon the Corn Laws? How could antislavery help ensure stability while also accommodating society to political and economic change? Antonio Gramsci defined "hegemony," in the words of his biographer, as "the predominance, obtained by consent rather than force, of one class or group over other classes"; or more precisely, "the 'spontaneous' loyalty that any dominant social group obtains from the masses by virtue of its social and intellectual prestige and its supposedly superior function in the world of production." The paramount question, which subsumes the others, is how antislavery reinforced or legitimized such hegemony.⁹

Unlike Foucault, Davis was confident that humanitarianism, or at any rate its antislavery component, represents an authentic and "remarkable shift in moral consciousness . . . a momentous turning point in the evolution of man's moral perception and thus in man's image of himself." Like Foucault, however, Davis insisted that the new sensibility "did not spring from transcendent sources." Rather its origin, he said, lies in "the ideological needs of various groups and classes."¹⁰ Davis achieved a highly nuanced view of the reformers' motivation by creating in the reader's mind a tense double image in which reformers appear not only as free moral actors, moved by ethical considerations of which they are fully conscious, but also as unwitting agents of class interest, moved by social needs that worked "over their heads" and were scarcely (if at all) accessible to consciousness. As long as one assumes, as Davis apparently did, that class interest is the only important link between base and superstructure, this juxtaposition of contradictory images is perhaps the only way to ward off reductionism and to do justice to the insolubility of the old problem of free will and determinism.

In an earlier book, *The Problem of Slavery in Western Culture*, Davis identified four major intellectual transformations that set the stage for an antislavery movement. Primitivist currents of thought permitted at least a momentary ambivalence about the superiority of European civilization. The evangelical movement in Protestantism dramatized the dangers of moral complacency even as the latitudinarian reaction against Hobbesian and Calvinistic views of man popularized an ethic of benevolence. Secular social philosophers from Hobbes to Montesquieu stripped away many of the previous sanctions for slavery and moved closer to a rejection of the institution, though Bodin was the only one who actually condemned it. These developments came to a practical focus in the affairs of the Quakers, the only perfectionist sect spawned by the revolutionary turmoil of seventeenth-century England that found a way to compromise and thus to survive and prosper. By the mid-eighteenth century, the Quakers were both representatives of the most radical strand of the Protestant tradition and figures in the vanguard of the development of capitalism and industrial society. The Quakers supplied a natural pivot for Davis's analysis as he turned in his second book away from the history of ideas to "more material considerations which helped both to shape the new moral consciousness and to define its historical effects."¹¹

⁹ Davis quoted John M. Cammet; *ibid.*, 348–49.

¹⁰ *Ibid.*, 41–42.

¹¹ *Ibid.*, 48.

Here in the material substructure Davis found the two principal threads of argument that he followed throughout his second volume. First, he explored "the ideological functions and implications of attacking this symbol of the most extreme subordination, exploitation, and dehumanization, at a time when various enlightened elites were experimenting with internalized moral and cultural controls to establish or preserve their own hegemony."¹² The second theme, less relevant to the purposes of this essay, traced the geopolitical and international economic considerations that led Great Britain to tolerate the annihilation of a species of property within its imperial borders and to exert military force against the slave trade, whether conducted by foreign nationals or its own citizens.

Davis turned away from the history of ideas in his second volume and opened the door to a much less voluntaristic and rationalistic mode of explanation than that tradition has thought acceptable. Yet his conversion was incomplete and uncertain. Characteristically, his strongest assertions explaining superstructural developments by reference to substructural ones are immediately followed by reservations that accumulate nearly to the point of contradiction. For example, Davis attributed receptivity to antislavery ideology to "profound social changes" connected with "the rise of new classes and new economic interests." But in the following sentence he declared that "this ideology emerged from a convergence of complex religious, intellectual and literary trends—trends which are by no means reducible to the economic interests of particular classes, but which must be understood as part of a larger transformation of attitudes toward labor, property and individual responsibility."¹³ Davis came closer in this passage than anywhere else to a recognition of the role played by cognitive style, yet, having almost said that class interest cannot be the exclusive link between humanitarianism and capitalism, he named no alternative link.

Davis's interpretation is not reductionist. He forthrightly rejected the argument that "Quaker abolitionists were governed by 'economic interest' in the sense that they stood to profit from the destruction of the slave trade or a weakening of the plantation system."¹⁴ Instead of direct profit, the relevant interests were those of an entrepreneurial class preoccupied with problems of unemployment and labor discipline. And Davis was acutely aware that even this argument "must be developed with considerable care to avoid the simplistic impression that 'industrialists' promoted abolitionist doctrine as a means of distracting attention from their own form of exploitation."¹⁵

Although Davis denied the existence of any crude cause-and-effect relationship between the needs of capitalists and the attack on slavery, he assigned great importance to a more subtle linkage based solidly on class interest. The heart of his analysis lies in the claim that, "as a social force, antislavery was a highly selective response to labor exploitation. It provided an outlet for demonstrating Christian concern for human suffering and injustice, and yet thereby gave a certain moral insulation to economic activities less visibly dependent on human suffering and injustice."¹⁶ Elsewhere, speaking of long-range consequences rather than immedi-

¹² *Ibid.*, 49.

¹³ *Ibid.*, 82.

¹⁴ *Ibid.*, 251.

¹⁵ *Ibid.*, 455.

¹⁶ *Ibid.*, 251.

ate intentions, he concluded that the abolitionist movement helped "clear an ideological path for British industrialists" and noted that, by exaggerating the harshness of slavery, abolitionists "gave sanction to less barbarous modes of social discipline." In the same breath Davis credited the abolitionist movement with breeding "a new sensitivity to social oppression" and providing a "model for the systematic indictment of social crime."¹⁷

The tendency of protests against chattel slavery to overshadow the evils of "wage slavery" had special significance for Davis because of the extraordinary role that Quakers played in the early antislavery movement. The Quaker reformers who were so prominent in antislavery and every other humanitarian endeavor of the age were often fabulously successful businessmen who epitomized the Protestant ethic and the capitalist mentality. Either directly or through close family connections they were deeply involved in industry, shipping, banking, and commerce; they knew firsthand the task of devising new modes of labor discipline to replace older methods of social control. As members of an entrepreneurial class confronted by an "unruly labor force" prone to "uninhibited violence" and not yet "disciplined to the factory system," late eighteenth-century reformers had strong incentives to formulate an ideology that would "isolate specific forms of human misery, allowing issues of freedom and discipline to be faced in a relatively simplified model."¹⁸

As these statements illustrate, Davis relied heavily on the explanatory power of class interest as the driving force behind ostensibly disinterested reforms. Apart from explicit denials of reductionism, what prevents his account from reducing the humanitarian sensibility to a reflexive instrument of the class struggle is his often-repeated conviction that the reformers were generally unaware of the interested character of their ideology and unable to see that it played a role in furthering the hegemony of their own class. The reformers, wrote Davis, "*unwittingly* drew distinctions and boundaries which opened the way, under a guise of moral rectitude, for unprecedented forms of oppression." He said of David Barclay and other Quakers that it was "*inconceivable*" to them "that English servants were in any sense unfree," and it was "*unthinkable* that an attack on a specific system of labor and domination might also validate other forms of oppression."¹⁹ In Davis's opinion, the formalistic conception of human freedom that enabled people militantly opposed to slavery to ignore the plight of the impoverished factory laborer and to turn their back on the ex-slave once he was legally free was powerful enough to constrain the vision even of the more radical abolitionists. "At issue, then," Davis concluded, "are not conscious intentions, but the social functions of ideology."²⁰

By insisting that the reformers were unaware of the hegemonic function served by their ideology, Davis opened a crucially important space between their intentions and the long-term consequences of their ideas and activities. It is mainly this zone of indeterminacy and free play that keeps his account clear of the reductionist and conspiratorial overtones that have so often plagued the social control argument. Yet it is also indispensable to Davis's purpose that the gap between intentions and

¹⁷ *Ibid.*, 466–68.

¹⁹ *Ibid.*, 253, 350 [My emphasis].

¹⁸ *Ibid.*, 241, 252, 254.

²⁰ *Ibid.*, 266 n., 350.

consequences not grow too wide, for, if the aid and comfort that abolitionism gave to capitalist hegemony was utterly unrelated to the intentions of the reformers, or if it was related only in an incidental or accidental way, Davis would have to abandon his conclusion.²¹ In order to conclude (as he did) that the attack on slavery was crucially shaped by the needs and interests of the rising class to which the reformers belonged, capitalist hegemony cannot be merely one among the many unintended consequences of reform. The category of unintended consequences is too loose to supply either the ethical or the causal quality that his explanation requires. After all, when Hank Aaron hit a home run, one of the consequences of the act was to put a new baseball into the hands of some lucky spectator. But this was not what Aaron intended by swinging at the ball, and we do not credit him with generosity because of the "gift" he bestowed on the spectators (nor would we have blamed him if the ball had struck one of them on the head.) If the aim of furthering capitalist hegemony entered into the intentions of the abolitionists no more significantly than the aim of gift giving figured in Aaron's mind as he swung at the ball, we would not feel that humanitarianism had anything important to do with the rise of capitalism—not, at least, as long as we assume that class interest is the only way to link the two. To say that a person is moved by class interest is to say that he *intends* to further the interests of his class, or it is to say nothing at all.²²

The intention need not be simple, of course. What is wanted, as Davis himself put it, is a way to show that the reformer's thinking was "rooted . . . in the needs of a social group" yet not "reducible" to them.²³ The problem (faced by Davis and anyone else striving to formulate a nonreductive explanation based on class interest) is that interest explains the conduct of reformers only by reducing it, revealing beneath the pretty surface of laudable intentions another layer that better accounts for their reforms. Interest explains much, but it explains by reduction. To shy away from the reductive step is to sacrifice explanatory force. No amount of cautious language can overcome the trade-off built into the logic of this kind of explanation.

²¹ *Ibid.*, 350: "The antislavery movement, like [Adam] Smith's political economy, reflected the needs and values of the emerging capitalist order. Smith provided theoretical justification for the belief that all classes and segments of society share a natural identity of interest. The antislavery movement, while absorbing the ambivalent emotions of the age, was essentially dedicated to a practical demonstration of the same reassuring message."

²² Although in my opinion he finally skirted its inescapably reductive implications, Raymond Williams fully concurred that the social control explanation cannot do without intention; Williams, "Base and Superstructure in Marxist Cultural Theory," *New Left Review*, 82 (1973): 3–16. Speaking of formulations like Georg Lukacs's that stress the "totality" of social practices rather than a layered image of base and superstructure, Williams said, "It is very easy for the notion of totality to empty of its essential content the original Marxist proposition." The danger, wrote Williams, is that of "withdrawing from the claim that there is any process of determination. And this I, for one, would be very unwilling to do. Indeed, the key question to ask about any notion of totality in cultural theory is this: whether the notion of totality includes the notion of intention. . . . Intention, the notion of intention, restores the key question, or rather the key emphasis." In order for an interpretation to be called "Marxist," Williams believed it should at least depict the "organization and structure [of society] . . . as directly related to certain social intentions, intentions by which we define the society, intentions which in all our experience have been the rule of a particular class." *Ibid.*, 7. My complaint, of course, is not that Davis, by relying on a soft form of intention, has veered too far from any "original proposition" but that he has clung to a greater degree of intention than he seems comfortable with, and a greater degree than the evidence can substantiate. The rival scheme of explanation I advocate retains the claim that there is a "process of determination" but deliberately abandons the claim of intentionality.

²³ Davis, *The Problem of Slavery in the Age of Revolution*, 349.

Unwilling to give up the explanatory force of class interest, yet uncomfortable with its tendency to undercut the authenticity of stated intentions, Davis, like many other recent scholars, resorted to the "soft" form of intentionality embodied in the notion of self-deception. By so doing he adopted a concept that bids fair to become the keystone of an imposing historiography constructed by Raymond Williams, E. P. Thompson, Eugene Genovese, and many other scholars who have drawn theoretical inspiration from the work of Gramsci. The notion of self-deception or some close equivalent of it has played an important role in the efforts of the "cultural school" to escape the criticism that doomed an earlier and more "positivist" tradition in Marxian historiography. The question I wish to raise is whether this conception can bear the explanatory weight thus thrust upon it.²⁴

Davis observed that "ideological hegemony is not the product of conscious choice and seldom involves insincerity or deliberate deception." He then quoted the sociologist Peter Berger, who said that "deliberate deception requires a degree of psychological self-control that few people are capable of. . . . It is much easier to deceive oneself."²⁵ In a passage that Davis did not quote but that would have suited his purposes equally well, the Marxist art critic Arnold Hauser made a similar point:

What most sharply distinguishes a propagandistic from an ideological presentation and interpretation of the facts is . . . that its falsification and manipulation of the truth is always conscious and intentional. Ideology, on the other hand, is mere deception—in essence self-deception—never simply lies and deceit. It obscures truth in order not so much to mislead others as to maintain and increase the self-confidence of those who express and benefit from such deception.²⁶

Unquestionably the concept of self-deception represents a major advance over the mechanistic formulations for which it substitutes. It virtually banishes the implication of conspiracy that so marred the work of Eric Williams, for example, and, if it does not grapple with the problem of free will at a very deep level, the concept at least does not pretend to have solved that problem by the discovery of an all-encompassing theory of determinism. In short, self-deception has the distinct merit of occupying the space between intention and consequence, precluding any rigid coupling of the two while maintaining a connection between them. The notion is, however, very slippery and, in spite of all its virtues, is not the best way to formulate the relationship between the abolitionists' intentions and the hegemonic consequences of their actions.

The problem with self-deception is not that it is a rare mental state or overly technical term. All of us can recall episodes in our lives when we ignored or denied

²⁴ On the two traditions in Marxist historiography, see Richard Johnson, "Edward Thompson, Eugene Genovese, and Socialist-Humanist History," *History Workshop*, 6 (1978): 79–100. On Gramsci, see Walter L. Adamson, *Hegemony and Revolution: A Study of Antonio Gramsci's Political and Cultural Theory* (Berkeley and Los Angeles, 1980), chap. 6; James Joll, *Antonio Gramsci* (New York, 1977), chap. 9; Thomas R. Bates, "Gramsci and the Theory of Hegemony," *Journal of the History of Ideas*, 36 (1975): 351–66; and Antonio Gramsci, *Selections from the Prison Notebooks of Antonio Gramsci*, ed. Quintin Hoare and Geoffrey Nowell Smith (New York, 1972).

²⁵ Davis, *The Problem of Slavery in the Age of Revolution*, 349.

²⁶ Hauser, as quoted in William A. Muraskin, "The Social Control Theory in American History: A Critique," *Journal of Social History*, 9 (1976): 566.

what now seems the plain and reprehensible meaning of our actions—moments when, to paraphrase what Sigmund Freud said about dreams, we knew what the consequences of our actions would be but did not know that we knew.²⁷ But the usefulness of the concept of self-deception to the historian is limited by two considerations. The first has to do with the ambiguous ethical import of acts committed by a person said to be deceiving himself; the second concerns the difficulty of distinguishing between cases of true self-deception and cases in which a person simply is either ignorant of some of the consequences of his actions or convinced that those consequences are incidental to his aim. Let us examine each problem in turn.

The ambiguity inherent in the idea of self-deception appears immediately when we ask what degree of ethical responsibility a person bears for acts the consequences of which he has deceptively concealed from himself. If we construe the term in a Freudian manner, knowledge of the unpalatable consequences is presumably hidden in the unconscious. Can a person will that what is unconscious become conscious? If not, he cannot be held responsible for acting on knowledge he does not know he possesses. Although a moralist in many ways, Freud had no patience with the idea that psychic events were undetermined or with the glib confidence that reason could master the unconscious. Making the unconscious conscious was for him the world-historical task of psychoanalysis, not the personal responsibility of ordinary individuals, unaided by therapeutic intervention.²⁸

But we also use the term self-deception to describe situations that are blameworthy. When we decide that an episode in our own past was a case of self-deception, we are embarrassed and feel regretful about it. The implication of our embarrassment is that we suspect we could have done other than we did, that by trying harder we could have become cognizant of the self-concealed consequences of our action and changed course accordingly. And of course the blame we attach to our own actions in such cases applies with at least equal force to the self-deceptions we think we see others commit.

What does it mean, then, to say that the abolitionists deceived themselves? That they could and should have overcome their self-imposed blindness? Or that they did all that could be expected, given the limitations of the only perspective available to them? The concept of self-deception is ambiguous enough to sustain either reading. More specifically, was Davis saying that chattel slavery and “wage slavery” were so similar that anyone opposing one ought to have opposed the other, that the formalism confining prevalent nineteenth-century definitions of freedom (among abolitionists and many others) to the mere absence of physical or legal constraint was so transparent that anyone who tried could have seen through it? This seems to be the clear implication of Davis’s contention that abolitionism was a highly “selective response to labor exploitation.” Yet as we have seen, Davis was not at all

²⁷ “For I can assure you that it is quite possible, and highly probable indeed, that the dreamer *does* know what his dream means: *only he does not know that he knows it and for that reason thinks he does not know it*”; Freud, *Introductory Lectures on Psychoanalysis*, trans. James Strachey (New York, 1977), 101. Freud likened the hidden knowledge to a person’s name that we know but cannot recall. His proof of the existence of such knowledge was, of course, hypnosis, and free association was the means of bringing it to light. Also see *ibid.*, 103, 110, 113.

²⁸ *Ibid.*, 106, 273–85.

comfortable with this implication; he also said that the reformers contributed to capitalist hegemony only “unwittingly” and that it would have been “unthinkable” and “inconceivable” for them to adopt less formalistic conceptions of liberty. These are strong words, and they play an important role in Davis’s account. They imply a far less voluntaristic image of the reformers, one that holds them essentially blameless for the limitations of their ideology—even though those limitations are said to have been self-imposed.²⁹ Without pursuing these conundrums any further, we can see that self-deception is an exceedingly spacious concept. I suspect that some readers welcome Davis’s use of the term precisely because it seems to insist on a fairly high degree of intentionality and responsibility, while others welcome it because it seems to let the abolitionists entirely off the hook. Life has irreducible ambiguities, but this is a case in which there may be greater ambiguity in the terms of representation than in the reality we seek to represent.

NOW LET US TURN to the second and more serious limitation on the usefulness of the concept of self-deception. Although the ethical import of self-deception is ambiguous, the intentionality of it is not. Self-deception implies intention, although not of the conscious variety. If it did not imply intention, it would not have served Davis’s purpose, which was to treat humanitarianism as a product in part of a specific kind of intention—advancing the interests of one’s own class.³⁰ The intentions implied by self-deception presumably operate unconsciously, and, as we have seen, this complicates their ethical significance but does not turn them into something other than intentions. Freudian theory is quite clear on this point: what resides in the unconscious are intentions (relegated to that nether region precisely because of their ugly character) that compete with and sometimes overwhelm the conscious intentions of the actor.³¹ One might argue that there are degrees of intentionality

²⁹ Davis’s claim that abolitionism was a “selective response to labor exploitation” is, of course, an echo of the criticism that radical labor leaders, such as William Cobbett, Richard Oastler, and Bronterre O’Brien, leveled against the abolitionists in their own time. What remains unclear, in spite of much recent discussion of the relation between abolitionism and the labor movement, is the exact basis of the labor critique. Did labor leaders work from a more advanced humanitarian perspective that really assigned equal importance to all varieties of exploitation, whether of slave or free labor (as Davis himself did)? Or did they simply assign a higher priority to the problems of wage laborers (nearby and racially similar) than to those of enslaved laborers (far away and racially different)? To what extent was the workingman’s criticism of abolitionism a pragmatic tactic for drawing attention to his own cause rather than a considered judgment of the equivalence of exploitation in the two cases? These are not easy questions to answer for the period before the 1840s, and they become easy then only in the case of a few extreme figures, like Marx and Engels, whose perspective clearly embraced (albeit abstractly) a wider world of suffering than did that of the abolitionists. Moreover, as we shall see in the first of two hypothetical exercises, even if we conclude that labor spokesmen really did “transcend” the limiting conventions of the day, this would not justify us in thinking that it would have been easy for their contemporaries to follow suit, or that their contemporaries would have done so but for self-deception. On labor and abolition, see Patricia Hollis, “Anti-Slavery and British Working-Class Radicalism in the Years of Reform,” in Bolt and Drescher, *Anti-Slavery, Religion, and Reform*, 295–315; Eric Foner, “Abolitionism and the Labor Movement in Antebellum America,” in *ibid.*, 254–71; Jonathan A. Glickstein, “‘Poverty is not Slavery’: American Abolitionists and the Competitive Labor Market,” in Lewis Perry and Michael Fellman, eds., *Antislavery Reconsidered: New Perspectives on the Abolitionists* (Baton Rouge, 1979), 195–218; Marcus Cunliffe, *Chattel Slavery and Wage Slavery: The Anglo-American Context, 1830–1860* (Athens, Ga., 1979); and Betty Fladeland, “‘Our Cause Being One and the Same’: Abolitionists and Chartism,” in Walvin, *Slavery and British Society*. In *Slavery and British Society*, see the essays by Drescher and Walvin that stress the great popularity of the abolitionist cause in England, even outside middle-class ranks.

³⁰ Raymond Williams’s comments on intentionality, quoted in footnote 22, above, are pertinent here as well.

³¹ Freud, *Introductory Lectures on Psychoanalysis*, 40–41, 61.

and that the self-deceiver's is less than the deliberate actor's, but in the last analysis the person deceiving himself must be said to know basically what the outcome of his action will be and to desire it (even though he may not be aware that he knows and desires it). Otherwise, the person who deceives himself would be no different from the person who through incomplete knowledge simply fails to anticipate all the consequences of his acts. That would make the term utterly vaporous. The person who deceives himself about the consequences of his actions knows a great deal more about them than the person who is ignorant of them.

Underlying any explanation built on the notion of self-deception, then, is an implicit claim to have successfully reconstructed the historical actor's unconscious intention. The claim is a bold one. Even conscious intentions are notoriously difficult to nail down in the absence of explicit statements of purpose (or even in their presence, for that matter), and unconscious intentions are much more problematical. By definition, they leave no direct empirical trace. One can only hypothesize their existence from the "goodness of fit" between the actor's interests (as reconstructed by the analyst) and certain consequences that follow from his actions. The reasoning process behind such hypothesizing is treacherous. After all, every act (or failure to act) has consequences both proximate and remote that are potentially infinite in number and extension. This is the stuff of which debates about the influence of horseshoe nails and Cleopatra's nose are made. It is altogether too easy to pluck from the ever-widening stream of consequences some that fit our reconstruction of the actor's interests and repackage these as his unconscious intention. Even if we assume that we know his interests, a question remains: what empirical evidence could, even in principle, confirm that the observed "goodness of fit" is more than coincidental?³²

The impossibility of confirming our hunches about the existence of unconscious intentions is only an aspect of a larger problem: the absence, even in principle, of any empirical evidence that would permit us to distinguish between the *unconsciously intended consequences* that the self-deception explanation requires and the *unintended consequences* that make up so much of what happens in human affairs. To return to an earlier analogy, when Hank Aaron hit a home run, we felt confident that the spectator's acquisition of a baseball was not Aaron's intention, even though it was unquestionably a consequence of his action. Our confidence might have yielded to confusion, however, if we had grounds for believing that Aaron had an incentive for currying the favor of fans by giving them gifts in this manner. In that unlikely

³² I pass hastily over a deep abyss—the glib assumption so characteristic of modern scholarship that a person's "interests" are readily identifiable and constitute a complete explanation of his conduct. In fact, the term is utterly elastic. There is no human choice that cannot be construed as self-interested. As a limiting case, consider a situation in which persons A and B happen on a burning house; person A dives into the flames to save the screaming occupants, while B refuses to endanger himself. B's selfishness is plain to see, but A's conduct can also be understood to reflect "enlightened" self-interest. The difference, one can argue, is simply that in A's subjective scheme of valuation, physical safety counts for less than social approbation. So he, like B, pursues what interests him most. (There is even a perspective from which one might prefer B's plainly selfish act to A's, on the grounds that B at least did not strive for the potentially "hegemonic" pleasure of achieving moral supremacy over another.) Needless to add, I am deeply troubled by any perspective on human affairs that obscures the radical moral superiority of A's choice over B's. The breath-taking scope of the metatheoretical issues at stake here is evident in the pivotal role that debates over the very possibility of altruistic action in a Darwinian universe have had in recent discussions of evolutionary theory and sociobiology. See Arthur L. Caplan, ed., *The Sociobiology Debate: Readings on Ethical and Scientific Issues* (New York, 1978), 213–26, 254, 308.

circumstance, we might suspect, but could never confirm, that gift giving was a significant, even though unconscious, part of his intention. Even if we could be sure that he was aware of the gift-giving consequences of his home run (say, by having called it to his attention as he strode to the plate), we could not confirm our suspicion, for all of us are always aware (or can easily be made aware) of consequences of our actions that fall outside intention in spite of our awareness of them. Knowing that a certain consequence will follow from one's actions does not necessarily make the production of that consequence part of one's intentions. The immense category of unintended consequences includes not only events of which the actor is completely ignorant but also events of which the actor may be aware but which social convention nonetheless classifies as unintentional—as incidental concomitants of his action (like Aaron's "gift" to the spectator) rather than the aim of it. Given the immensity of this category of events, and the absence of any empirical means by which unintended consequences can be distinguished from unconsciously intended consequences, one may well doubt that the social control argument gains anything of substance by relying on the notion of self-deception.

Among all these liabilities, the really fatal flaw of the self-deception argument is its obliviousness to the paramount role played by convention in all judgments of moral responsibility. Once we understand the inescapable part that conventions play in channeling and limiting responsibility, it becomes apparent that imputations of unconscious intention (empirically unverifiable anyway) are gratuitous in the case of the abolitionists. What I aim to show in the next section of the essay is that abolitionists did not need to hide anything from themselves. All of us, no matter how humane, disown responsibility every day for known consequences of our own acts (and omissions) that are far more horrifying than those the abolitionists disowned when they chose to help slaves rather than wage workers. Keeping a clear conscience in spite of being causally involved in the suffering of others does not require self-deception. There was nothing distinctively selective about the abolitionists' preoccupation with chattel slavery: all humane action entails "selectivity." What enables us all—the abolitionists in their day and you and me in ours—to maintain a good conscience, in spite of doing nothing concretely about most of the world's suffering, is not self-deception but the ethical shelter afforded to us by our society's conventions of moral responsibility. These conventions allow us to confine our humane acts to a fraction of suffering humanity without feeling that we have thereby *intended*, in any way, or *caused*, in any morally significant way, the evils that we leave unrelieved.

The reader may well suspect sleight-of-hand at this stage of my argument. Are not these social conventions merely collective forms of self-deception? And might they not conceal widely held unconscious intentions of the sort that Davis and many other historians have postulated? I think even a skeptical reader will be persuaded by the following arguments that the burden of proof rests heavily on anyone who makes these claims. The conventions I have in mind are always open to criticism from the vantage point of rival conventions (we certainly need not admire those of any earlier generation), but they are not the transient reflexes of any social interest, and not even the most extraordinary feats of moral gymnastics would permit a

person to transcend the limits of all such conventions, thereby becoming perfectly humane and invulnerable to the charge of "selectivity."

To show that this is so I ask the reader to participate in two mental exercises, which have a twofold purpose. First, they explore the problem of "selectivity" and highlight the role that conventions play in judgments of moral responsibility, thus wrapping up my criticisms of the self-deception argument and the social control thesis that it supports. Second, the longer of the two exercises, the "case of the starving stranger," brings into view the crucial anatomical features of the historical process that I believe gave rise to the modern humanitarian conscience and, therefore, paves the way for an alternative to the social control explanation, one that does not rely on class interest as the connecting link but nevertheless firmly connects capitalism with the emergence of the humanitarian sensibility.

FIRST, WE CAN ILLUMINATE THE CHARGE of "selectivity" and self-deception by imagining ourselves to be in a situation analogous to that of the abolitionists, men and women living in the midst of a great change in moral sensibility. The crux of the analogy addresses the intriguing problem to which Davis returned again and again: why were the abolitionists so slow, at best, to concern themselves with the misery of free workers. That they *could* have been quicker to see through the formalism so characteristic of nineteenth-century liberalism and *could* have acknowledged the similarity of chattel slavery to wage slavery is sufficiently demonstrated by those contemporary reformers, such as Owen, Fourier, Marx, and Engels, who actually did so. But the fact that abolitionists could have been more concerned with the plight of the free laborer does not justify the speculation that they really knew they should be and only failed to be because they deceived themselves.

Imagine that fifty or a hundred years from now the world is swept by another great shift in moral sensibility—this time a wave of revulsion against man's carnivorous way of life. The possibility is not, after all, so far-fetched: philosophers have long debated what duties man owes to other sentient beings; antivivisection is an issue that has moved many; hard-boiled legislators pass statutes protecting endangered species; prominent publications like the *New York Review of Books* occasionally run articles on the problematical ethics of eating flesh. Dietary regulations occupy an important place in many religions, and whole Asian cultures have for extended periods regarded vegetarianism as a prerequisite to the ethical life. Moreover, we have in our midst many people who practice vegetarianism, providing a living model to those of us (including myself) who continue to eat meat.

If vegetarianism should someday become the mainstream point of view, how would the historians among our vegetarian descendants view us, their carnivorous ancestors? I suppose they would ask much the same questions about us that Davis asked about the abolitionists. How can we draw such an arbitrary line between human misery and the misery of nonhuman, but certainly sentient, creatures? Is not our comparatively intense concern for oppressed human beings a highly selective response to the general problem of predation, one that provides an outlet

for demonstrating concern for suffering yet thereby gives a certain moral insulation to even more ruthless predatory practices in our society? Surely it would be tempting from the vegetarian point of view to say that all our busy efforts to alleviate human misery serve to isolate specific forms of suffering, thereby allowing issues of moral responsibility to be faced in a relatively simplified model. And they might even say that exaggeration of the harsh consequences of poverty, the pain of discrimination, the penalties of class, and the horrors of human warfare allows our most dedicated reformers to give tacit sanction to the systematic slaughter of nonhumans.

Readers who refrain from eating meat on ethical grounds may wish to endorse this interpretation insofar as it is an embodiment of their own anticarnivorous values. But I think even they would agree that the vegetarian historian's interpretation is flatly wrong on two counts. First, it errs in tacitly assuming that, because some twentieth-century people are ethically opposed to eating meat, others must know in their hearts that it is wrong and can only maintain a clear conscience by deceiving themselves. It is conceivable to me that I may someday become convinced that eating meat is wrong, but by no stretch of the imagination can I persuade myself that in some way I already know that it is wrong. I continue eating meat not because I am deceiving myself but because whatever suffering my dietary preference causes falls outside the conventions of responsibility by which I presently live.³³ Second, the vegetarian historian would err even more egregiously if he supposed that we carnivores, in order to sustain our self-deceptions, busy ourselves with projects to alleviate human suffering as a means (conscious or not) of putting animal suffering out of mind. My indifference to animal suffering may depend on seeing only the end product of the butcher's work, but it does not depend on my all-too-infrequent efforts to alleviate human suffering. The vegetarian historian's error in imputing self-deception to us, his humane but unrepentantly carnivorous ancestors, is duplicated when we impute self-deception to the abolitionists on account of their failure to extend help to wage slaves.

Now let us proceed to a more elaborate exercise designed to show how inescapable conventions are in the allocation of moral responsibility, and how the conventions themselves change in time. Let us call this the "case of the starving stranger." As I sit at my desk writing this essay, and as you, the reader, now sit reading it, both of us are aware that some people in Phnom Penh, Bombay, Rangoon, the Sahel, and elsewhere will die next week of starvation. They are strangers; all we know about them is that they will die. We also know that it would be possible for any one of us to sell a car or a house, buy an airline ticket, fly to Bombay or wherever, seek out at least one of those starving strangers, and save his life, or at the very least extend it. We could be there tomorrow, and we really could save him. Now to admit that we have it in our power to prevent this person's death by starvation is to admit that our inaction—our preference for sitting here, reading and writing about moral responsibility, going on with our daily routine—is a

³³ A defender of the self-deception argument might ask how I can claim to know whether I am deceiving myself or not, to which the appropriate reply is that, in the absence of any possibility of empirical demonstration, my claim, based on introspection, has at least as much merit as any opposing claim.

necessary condition for the stranger's death. But for our refusal to go to his aid, he would live.

This means that we are causally involved in his death. Our refusal to give aid is one of the many conditions that, all together, make up what John Stuart Mill called the cause "philosophically speaking" of this evil event. Now to say that we are causally involved is, of course, not to say that our failure to act is "the cause" of his death: it is only one among many conditions, and not every condition is properly regarded as "the cause." But the troubling fact remains that *but for* our inaction this evil event would not occur.³⁴

Why do we not go to his aid? It is not for lack of ethical maxims teaching us that it is good to help strangers. Presumably we all subscribe to the Golden Rule, and certainly if we were starving we would hope that some stranger would care enough to drop his daily routine and come to our aid. Yet we sit here. We do not do for him what we would have him do for us. Are we hypocrites? Are we engaged in self-deception? Do we in any sense *intend* his death?

I think not—unless, of course, we wish to stretch the meaning of intention way beyond customary usage, so that it indiscriminately lumps together premeditated murder with a failure to avail ourselves of an opportunity to do good. There is much more to say about the way we arrive at judgments of both causation and intention, but for my purposes it is enough to observe that the limits of moral responsibility have to be drawn somewhere and that the "somewhere" will always fall far short of much pain and suffering that we could do something to alleviate. What is crucially important to see is that we never include within our circle of responsibility all those events in which we are causally involved. We always set limits that fall short of our power to intervene. Whatever limits we do set can therefore always be challenged and made to look arbitrary or "selective" by insistent questioning—for they are finally nothing more than conventions. Good reasons can be given for preferring some conventions to others, but there is no escaping convention itself and even a degree of arbitrariness in our choice of which to accept. The necessity for being selective is built into the nature of the problem. Even the person who tries to extend his limits to encompass all those events in which he is causally involved will, in his futile efforts to save all the starving strangers in the world, have to choose whether to go first to Bombay or Calcutta and whether to begin with person X or person Y. These choices will appear no less arbitrary (at least to the stranger not chosen) than the convention that permits you and me to exclude this predictable consequence of our inaction from the category of intention, and to sit here with only a pinprick of guilt as we contemplate our involvement in the stranger's death.³⁵

³⁴ Mill, as quoted in H. L. A. Hart and A. M. Honoré, *Causation in the Law* (London, 1959), 16. Hart's analysis is the foundation of much of what I say here, though I am strongly attracted to Joel Feinberg's amendments to Hart's excessively voluntarist perspective. See Feinberg, "Causing Voluntary Actions," in Feinberg, *Doing and Deserving: Essays in the Theory of Responsibility* (Princeton, 1970), 152–86. It is noteworthy that much of Hart's magisterial volume is an inquiry into "the principles governing the selection we apparently make of one of a complex set of conditions as the cause" and that when he inaugurated that inquiry it was "a problem scarcely mentioned before in the history of philosophy"; Hart and Honoré, *Causation in the Law*, 16.

³⁵ Among these sheltering conventions are those that permit us to feel that we have done our part by making

Curiously, our feeling of responsibility for the stranger's plight, though nowhere near strong enough to move us to action, is probably stronger today than it would have been before the airplane. If William Wilberforce had faced this question in 1800, he at least could have begged off on the grounds that the sea voyage to India was long and costly and he might die enroute. This suggests that new technology—using that word broadly to refer to all means of accomplishing our ends, including new institutions and political organizations that enable us to attain ends otherwise out of reach—can change the moral universe in which we live. Technological innovation can perform this startling feat, because it supplies us with new ways of acting at a distance and new ways of influencing future events and thereby imposes on us new occasions for the attribution of responsibility and guilt. In short, new techniques, or ways of intervening in the course of events, can change the conventional limits within which we feel responsible enough to act. Imagine that we have at our disposal an as yet uninvented technology, far more advanced than the airplane, that will enable us to save the starving stranger with minimal expenditure of time and energy, no disruption of our ordinary routine. If we could save him by just reaching out to press a button, then a failure to act would become indefensible. What convention previously enabled us to regard as an acceptably incidental concomitant of our inaction would then be transformed into heinous neglect or even—arguably—an intention to do harm. No convention could save us from responsibility then. And notice that this drastic change in our operative sense of responsibility could be brought about without any change at all in our ethical convictions. All of our ideas, every abstract formulation of moral obligation could remain the same; the only change needed to get us over the threshold of action is an expansion of the range of opportunities available to us for shaping the future and intervening in other lives. The latter point constitutes an especially telling objection to those who believe that humanitarianism can be explained merely by pointing to the proliferation of sermons and other texts on the importance of love and benevolence.

These “ways of intervening in the course of events” and “opportunities for shaping the future” play such an important role in the history of moral responsibility and will occupy such a prominent place in my analysis that they deserve a distinctive label. The word “technique” carries misleading connotations—of superficialities of style, on the one hand, and of highly organized bodies of scientific knowledge, on the other. What we need is a word that suggests the full range of practical know-how about cause-and-effect connections, from that required to put a man on the moon, at the upper end of the scale, to that needed to get in a harvest on time or mobilize the manpower and material needed to run a blacksmith's shop, at the lower and much more important end of the scale.

donations to charitable organizations, or by committing ourselves to a political movement that, if it should triumph, would, we trust, alleviate the suffering in question. The latter route to a good conscience is especially vulnerable to the charge of self-delusion, and neither of these routes can seem anything but arbitrary and “selective” from the standpoint of the stranger who starves next week. Yet these are the best choices available to us. Surely it is better to send an annual check to CARE, or even merely to adopt a political rhetoric that condemns maldistribution of wealth, than to do nothing at all. How much better remains far more open to question than any of us like to think.

The philosopher Douglas Gasking not only suggested a suitably homely name in his essay "Causation and Recipes" but also explained why this category of practical formulas for getting things done is so important. The very idea of causation, in its most fundamental or primitive sense, is, according to Gasking, "essentially connected with our manipulative techniques for producing results." Not science but plain *recipe* knowledge, or technique in its most inclusive sense, is the wellspring of causal thinking. "A statement about the cause of something," wrote Gasking, "is very closely connected with a recipe for producing it or for preventing it."³⁶ Although one can think of science as an elaboration of recipe knowledge, resulting from the substitution of open-ended inference-licenses for recipes, it is what Gasking called the "producing-by-means-of relation" that underlies causal thinking, and this relation emerges from man's most basic efforts to sustain life.

Men discovered that whenever they manipulated certain things in certain ways in certain conditions certain things happened. When you hold a stone in your hand and make certain complex movements of arm and fingers the stone sails through the air approximately in a parabola. When you manipulate two bits of wood and some dry grass for a long time in a certain way the grass catches fire. When you squeeze an egg, it breaks. When you put a stone in the fire it gets hot. Thus men found out how to produce certain effects by manipulating things in certain ways: how to make an egg break, how to make a stone hot, how to make dry grass catch fire, and so on.³⁷

What makes recipe knowledge important for the historian trying to understand the rise of humanitarianism is that neither causal perception nor feelings of moral responsibility can exist in the absence of appropriate recipes. One simply cannot see a human act (or omission) "A" as the cause of event "E" unless one possesses a recipe for producing events *like* "E" by means of acts (or omissions) *like* "A." And where the very possibility of causal perception is lacking, there can be no feelings of moral responsibility. By the same token, other things remaining equal, an enhancement of causal perception by the introduction of new or more far-reaching recipes can extend moral responsibility beyond its former limits.

Armed with this understanding of the dependence of moral conventions on recipe knowledge and causal perception, we can find in the case of the starving stranger many clues and research suggestions for the historian who wants to explain the origins of humanitarian sensibility. Although the clues point strongly in the direction of capitalism, they do not point toward class interest. To extract these clues it will be helpful to formalize the case of the starving stranger, recasting it as a set of preconditions that must exist before people will go to the aid of strangers.

There are four preconditions to the emergence of humanitarianism as a historical phenomenon. First and most obvious, we must adhere to ethical maxims that make helping strangers the right thing to do before we can feel obliged to aid them. If our ethical convictions permit us to ignore the suffering of people outside our own family or clan, then there can be no basis whatever for the emergence of those activities and attitudes that we call humanitarian. Although adherence to

³⁶ Gasking, "Causation and Recipes," *Mind*, 64 (1955): 483. Also see Hart and Honoré, *Causation in the Law*, 26, 29, 69.

³⁷ Gasking, "Causation and Recipes," 486, 482.

appropriate maxims is indispensable, the case of the starving stranger shows that it is not enough by itself to provoke humane behavior. The Golden Rule alone provides a sufficient ethical basis for our deciding right now to get up and go to Ethiopia, yet we remain seated.

A second precondition, also illustrated in the case of the starving stranger, is that we must perceive ourselves to be causally involved in the evil event. Once again, being causally involved does not mean that we regard ourselves as "the cause" but only that we recognize our refusal to act as a necessary condition without which the evil event would not occur. Along with this prerequisite goes the third. We cannot regard ourselves as causally involved in another's suffering unless we see a way to stop it. We must perceive a causal connection, a chain made up of cause-and-effect links, that begins with some act of ours as cause and ends with the alleviation of the stranger's suffering as effect. We must, in short, have a technique, or *recipe*, for intervening—a specific sequence of steps that we know we can take to alter the ordinary course of events. As long as we truly perceive an evil as inaccessible to manipulation—as an unavoidable or "necessary" evil—our feelings of sympathy, no matter how great, will not produce the sense of operative responsibility that leads to action aimed at avoiding or alleviating the evil in question.

Although the possession of such recipes sets the stage for going to the stranger's aid, even this is not enough. Today you and I have a recipe for getting to Ethiopia and preventing starvation, we admit that our failure to go is a necessary condition for the stranger's death, and we all subscribe to the Golden Rule. Yet here we sit.

The fourth precondition, the one that finally gets us into a psychological frame of mind in which *some* of us will feel compelled to act, is this: the recipes for intervention available to us must be ones of sufficient ordinarieness, familiarity, certainty of effect, and ease of operation that our failure to use them would constitute a suspension of routine, an out-of-the-ordinary event, possibly even an intentional act in itself.³⁸ Only then will we begin to feel that our inaction is not merely one among many conditions necessary for the occurrence or continuation of the evil event but instead a significant contributory *cause*.

To say that our refusal to aid the stranger only assumes causal status when it appears extraordinary against a background of ordinary recipe usage is to base ourselves securely in the existing literature on the philosophy and psychology of causal attribution, for that literature continually reaffirms that abnormality is the principal criterion that prompts us to single out certain events, acts, or conditions as causal.³⁹ The main reason you and I can go about our daily routine and not be

³⁸ The fourth precondition is not that we must have an ordinary, familiar, and certain recipe specifically tailored to the specific task at hand but that our ordinary, familiar, and certain recipe must be sufficiently *like* what is needed to inspire confidence that the task can be accomplished with only moderate adjustments of the available and proven means.

³⁹ "The notion, that a cause is essentially something which interferes with or intervenes in the course of events which would ordinarily take place, is central to the common-sense concept of cause, and at least as essential as the notions of invariable or constant sequence so much stressed by Mill and Hume"; Hart and Honoré, *Causation in the Law*, 27. Hart also assigned a special place to voluntary acts, but Feinberg argued that these are so often perceived as causal only because of their "abnormality." "The more expectable human behavior is, whether voluntary or not, the less likely it is to 'negative causal connection' [that is, to be seen as a cause rather than as the effect of some more remote cause]"; Feinberg, "Causing Voluntary Actions," 165. Also see N. R. Hanson, "Causal

overwhelmed with guilt about the stranger's plight is that the only recipe we have for going to his aid is far more exotic and more difficult to implement than the recipes we customarily use in everyday life. It involves a causal connection between his life and ours that is much more indirect, remote, and tenuous than the ones we habitually employ, so we do not regard our failure to act on the recipe as abnormal. None of us habitually liquidates major assets and departs on a moment's notice for the remotest parts of the globe in the pursuit even of selfish ends. If in our everyday routines we normally employed recipes as exotic as this one, then our failure to use it would begin to look extraordinary, and would therefore begin to assume causal status. And, once we begin to perceive our inaction as a cause of the stranger's suffering, then the psychological pressure to do something in his behalf can grow irresistible.⁴⁰

THESE PRECONDITIONS DRAWN FROM THE CASE of the starving stranger help clarify both the way in which revolutions in moral sensibility ought to be conceived and the way in which they are to be explained. First, we ought to construe major alterations of sensibility such as the rise of abolitionism as the result of shifts in the conventional boundaries of moral responsibility. Thus, what emerged in the century after 1750 was not, in the first instance at least, either a new configuration of class interests or a novel set of values geared to the hegemony of a rising class. Instead, the principal novelty was an expansion of the conventional limits of moral responsibility that prompted people whose values may have remained as traditional (and as unrelated to class) as the Golden Rule to behave in ways that were unprecedented and not necessarily well suited to their material interests. What happened was that the conventional limits of moral responsibility observed by an influential minority in society expanded to encompass evils that previously had fallen outside anyone's operative sphere of responsibility. The evils in question are of course the miseries of the slave, which had always been recognized but which before the eighteenth century had possessed the same cognitive and moral status that the misery of the starving stranger in Ethiopia has for us today.⁴¹

The question historians need to answer is why events such as the death of the

Chains," *Mind*, 64 (1955): 309: "We tend to be very selective about the sorts of things of which we ask, 'What is its cause?' This question is usually asked only when we are confronted with a *breach* of routine."

⁴⁰ Recipe knowledge stems from many sources. Because my aim in this essay is to link humanitarianism to the market, the recipe knowledge of immediate interest is that which originated in market transactions. But no recipe knowledge was more critical in the origins of humanitarianism and antislavery than that which originated in the special religious and political experience of the Quakers. Howard Temperley noted that, in the course of their struggles against tithe bills and other injurious legislation, Quakers by the 1730s had already mobilized quarterly, monthly, and local meetings into "a political machine of remarkable strength and sophistication . . . [combining] central direction with constituency action. Long before anyone else, Quakers had become adept at using a broad range of techniques designed to exert extraparlimentary pressure, including mass petitioning, lobbying, drawing up voting lists, and obtaining pledges." Temperley, "Anti-Slavery," in Patricia Hollis, ed., *Pressure from Without: In Early Victorian England* (London, 1974), 31. Quakers were not only adept at manipulating public opinion and Parliament but also knew more than anyone else about shaping the traits of individual character, a kind of recipe knowledge that was presupposed by the penitentiary and the "moral treatment" of the insane. See Richard T. Vann, *The Social Development of English Quakerism, 1655-1755* (Cambridge, Mass., 1969), chap. 1, and pp. 167-79, 204-08.

⁴¹ A section of *Las siete partidas*, thirteenth-century legal guidelines that influenced later legislation in Spain

starving stranger sometimes move out of the morally indifferent category of "unintended consequences," for which no one feels any operative sense of responsibility, and become matters for which certain people feel acutely responsible. Here lies the second lesson to be drawn from our list of prerequisites: once revolutions in moral sensibility are understood to result from shifts in the conventional boundaries of moral responsibility, the task of explanation can be seen to consist in finding the historical developments that exerted an outward, expansionary pressure on those conventional limits. Our set of preconditions also suggests what successful explanations might look like by alerting us to the intimate relationship between feelings of responsibility and perceptions of causal involvement. It suggests that the limiting conventions by which people live are a function of the range of events in which they perceive themselves to be causally involved, either by commission or omission and, further, that the range of events in which they perceive themselves to be involved is shaped by the number and the "power" or "reach" of the recipes for intervention that they and the people around them habitually use. What expands a person's horizons of causal involvement, and hence potentially expands also his limits of moral responsibility, is the routine employment of recipes of great complexity or temporal extension. So what the historian needs to look for are historical factors that give people heightened confidence in recipes they already have or encourage them to develop new recipes, or more complex recipes, or recipes that reach farther into the future. Every new recipe, or increase in the "reach" or complexity of recipes, extends the horizons of causal perception and thereby broadens the sphere within which a person may *potentially* feel himself to be the cause of an evil.

The word "potentially" requires special attention. We are not concerned with individual episodes of human kindness and decency—which I assume can occur anywhere, anytime—but with a sustained, collective pattern of behavior in which substantial numbers of people regularly act to alleviate the suffering of strangers. That, I take it, is what we mean by the emergence of a new humanitarian sensibility in the eighteenth century. Our aim, then, is to specify the minimum conditions that must be satisfied before this collective phenomenon can begin. Not all people who experience these conditions will become humanitarian reformers. In fact, most will not, and to account for those few who do we would have to look beyond the limits of this explanatory scheme to conventional biographical and historical details about the teachings to which they were exposed, their personal temperaments, the depth of their religious faith, their courage, and so on.

Although the form of explanation suggested here does not pretend to explain why certain individuals become humanitarian reformers and others do not, it does alert us to the distinctiveness of the moral experience of people who are equipped with a large repertoire of far-reaching recipes. Because such people feel compara-

and Spanish America (both of which accepted the institution of slavery), declared that "slavery is the most evil and the most despicable thing which can be found among men, because man, who is the most noble and free creature, among all the creatures that God made, is placed in the power of another"; Herbert S. Klein, "Anglicanism, Catholicism, and the Negro Slave," in Ann J. Lane, ed., *The Debate over Slavery: Stanley Elkins and His Critics* (Urbana, 1971), 142.

tively confident of their ability to intervene in the course of events and to shape the future at will, they are the prime candidates for the first historical appearance of that heightened feeling of causal involvement that is a prerequisite for humanitarianism. With the capacity to intervene goes the possibility of feeling obliged to take responsibility—but only the possibility. Many people, whose large endowment of recipe knowledge gives them the capacity to intervene, will not do so, or will do so only for the sake of self-aggrandizement, but some unusually sensitive individuals will be “trapped,” as it were, by their broadened horizons of causal involvement and feel compelled to go to the aid of strangers for whose misery they would previously have felt no more than passive sympathy. The explanatory approach recommended here does not pretend to plumb the mysteries of individual sensitivity or compassion. It does, however, offer a way to understand why even the most scrupulous and compassionate men and women did not feel obliged to go to the aid of suffering slaves before the middle of the eighteenth century.⁴²

Far from transforming acts of conscience into subtle reflexes of more fundamental drives, this explanatory approach permits us to give the humanitarian reformers full credit for their moral insight, their courage in the face of adversity, and their tenacity in uprooting entrenched institutions. They were consummate interpreters of a new moral universe. Yet it also enables us to see that their new interpretation was called forth by changes in the social and economic conditions of life and that, once the stage was set, similar measures almost certainly would have been carried out by other individuals if not by Wilberforce, Garrison, Phillips, and all the other formidable men and women who actually did the job. One need not pretend that human beings are uncaused causes in order to admire them.

⁴² In fact, this mode of explanation is perfectly compatible with the assumption that such individual personality traits as compassion and scrupulosity were as evident before 1750 as after. If being “good” consists—as I think it must, for the most part—of scrupulous adherence to the ethical maxims of one’s culture (such as the Golden Rule), within the operational limits prescribed by convention and the availability of techniques for intervention, then it makes perfectly good sense to say that people did not become “better” after 1750; they just adapted their conduct to a new set of conventions and capabilities. The argument thus does not rest on any assumption of moral progress, as that term is ordinarily understood, though it does assume that post-1750 conventions of responsibility embrace a wider range of suffering humanity and are in that sense superior to earlier ones.

Part 2 of this essay will be published in the June 1985 issue of the Review. The Editor.

Why Foreign Oil Companies Shifted Their Production from Mexico to Venezuela during the 1920s

JONATHAN C. BROWN

PETROLEUM WAS BOTH A BLESSING AND A CURSE to the Latin American nations in which it was found. The oil industry offered higher wages, provided an efficient fuel for transport and industry, and paid revenues that enabled the state to expand political, economic, and social programs. But, before 1930, petroleum development also meant that large foreign enterprises assumed de facto control of national resources to such a degree that governing elites in Latin America began to question whether they still directed their own destiny. Recent research on the oil history of Mexico and Venezuela illustrates well the conflict between nation-state and private foreign company.¹ One aspect, however, warrants further investigation: private industry's shift of interest and investments out of revolutionary Mexico and into Venezuela. Before the shift Mexico was second only to the United States in oil production. By 1930 it had fallen to seventh position, while Venezuela ranked third (see Table 1). What were the reasons for Mexico's decline and Venezuela's rise? Did British and U.S. oilmen conspire to punish a revolutionary Mexican government that was nationalizing oil properties? Were the forces of supply and demand in the

A grant-in-aid from the American Council of Learned Societies in 1983 made possible my research in London. Lynore E. Brown, Stanley R. Ross, John M. Tutino, and three anonymous readers for the *American Historical Review* provided suggestions on an earlier version that I delivered at the 1983 meeting of the American Historical Association in San Francisco. I dedicate this essay to the memory of Stanley R. Ross, scholar of Latin America and friend, who died in February.

¹ For some of the most recent studies, see B. S. McBeth, *Juan Vicente Gómez and the Oil Companies in Venezuela, 1908–1935* (Cambridge, 1983); George Philip, *Oil and Politics in Latin America: Nationalist Movements and State Oil Companies* (Cambridge, 1982); Rómulo Betancourt, *Venezuela: Oil and Politics* (Boston, 1979); Franklin Tugwell, *The Politics of Oil in Venezuela* (Stanford, Calif., 1975); Marcos Kaplan et al., *Petróleo y desarrollo en México y Venezuela* (Mexico City, 1981); Mira Wilkins, "Multinational Oil Companies in South America in the 1920s: Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, and Peru," *Business History Review*, 48 (1974): 414–46; Stephen G. Rabe, *The Road to OPEC: United States Relations with Venezuela, 1919–1976* (Austin, Tex., 1982); Friedrich Katz, *The Secret War in Mexico: Europe, the United States, and the Mexican Revolution* (Chicago, 1981); Lorenzo Meyer, *Mexico and the United States in the Oil Controversy, 1916–1942* (Austin, Tex., 1977); Merrill Rippy, *Oil and the Mexican Revolution* (Leiden, 1972); George W. Grayson, *Politics of Mexican Oil* (Pittsburgh, 1980); Robert Freeman Smith, *The United States and Revolutionary Nationalism in Mexico, 1916–1932* (Chicago, 1972); Nora Hamilton, *The Limits of State Autonomy: Post-Revolutionary Mexico* (Princeton, 1982); Esperanza Durán de Seade, "Mexico's Relations with the Powers during the Great War" (D.Phil. thesis, Oxford University, 1980); and N. Stephen Kane, "Corporate Power and Foreign Policy: Efforts of American Oil Companies to Influence United States Relations with Mexico," *Diplomatic History*, 1 (1977): 170–98.

international marketplace responsible? Was Venezuela a more beneficial climate for investment than Mexico?

Until recently the unavailability of oil company records has prompted historians to favor a political explanation: that revolutionary reforms drove oilmen from Mexico to Venezuela, whose dictator promoted private investment. Without such records scholars cannot deal with oilmen as businessmen responding to market as well as political conditions. But S. Pearson and Son, Ltd., of London has now made available the papers of El Aguila, the oil company it owned in Mexico. In addition, I have received, from the late Henrietta M. Larson, documents relating to the Latin American operations of Standard Oil Company of New Jersey and have researched the archives of Shell International Petroleum Corporation in London. U.S. State Department and British Foreign Office records have provided additional details on the political and economic policies of the oil firms. These corporate and diplomatic sources permit a reconstruction of capital investment in Mexico and Venezuela and a review of the timing and motivation of business decisions. Because the political history of the companies in Mexico and Venezuela is so widely known, I will emphasize the relatively neglected economic determinants of their actions.

IT IS CERTAINLY NO EXAGGERATION to state that foreigners developed Mexico's petroleum industry not for export but to satisfy domestic demand in Mexico itself. Beginning in the 1880s, Waters-Pierce Oil Company of St. Louis, a Standard Oil affiliate, nearly monopolized the export to Mexico of United States petroleum products, principally kerosene and lubricants.² Pierce lost that monopoly in the first decade of the twentieth century. Edward L. Doheny, a U.S. independent (unconnected with large companies), initiated the substitution of domestic petroleum production in Mexico for foreign imports. In 1901 Doheny's first well came in at El Ebano, but its product was heavy and viscous, unsuitable for kerosene or for export, especially at a time when the Spindletop, Texas, oil boom had depressed U.S. prices. Doheny first used the oil to pave the streets of Mexican cities. In 1905 he finally completed contracts to provide Mexican railways with fuel oil, which replaced imported coal. Doheny's El Ebano oilfield eventually fueled fifty locomotives of the central railway and the smelting operations of Mexico's foreign-owned mining industry.³

British businessman Sir Weetman Pearson, an engineer with long experience in Mexico, was the second independent to encroach on the Waters-Pierce monopoly. Having built the Tehuantepec railway, Pearson obtained the oil rights on the isthmus from the government of President Porfirio Díaz. Subsequently, the Mexican president encouraged British oil investments in order to counterbalance the dominant American interests. In 1907 Pearson attempted to purchase a one-

² Ralph W. Hidy and Muriel E. Hidy, *Pioneering in Big Business, 1882-1911: History of Standard Oil Company (New Jersey)* (New York, 1955), 222, 320, 448-51.

³ U.S., Congress, Senate, Committee on Foreign Relations, [hereafter, Senate Committee on Foreign Relations], *Investigation of Mexican Affairs*, 66th Cong., 1st sess., 1919, vol. 1, 227-29, 233; and Mira Wilkins, *The Emergence of Multinational Enterprise: American Business Abroad from the Colonial Era to 1914* (Cambridge, Mass., 1970), 123-24.

TABLE 1
Annual Oil Production of Mexico and Venezuela, 1904-30
(in thousands of barrels)

<i>Year</i>	<i>Mexico</i>	<i>Venezuela</i>
1904	126	
1905	251	
1906	502	
1907	1,002	
1908	3,933	
1909	2,714	
1910	3,634	
1911	12,553	
1912	16,558	
1913	25,696	
1914	26,235	
1915	32,911	
1916	40,546	
1917	55,293	120
1918	63,828	333
1919	87,073	425
1920	157,069	457
1921	193,398	1,433
1922	182,278	2,201
1923	149,585	4,201
1924	139,678	9,042
1925	115,515	19,687
1926	90,421	37,226
1927	64,200	64,400
1928	50,151	105,749
1929	44,688	137,472
1930	39,530	136,669

SOURCES: *The Oil Weekly*, March 30, 1928, p. 76; and American Petroleum Institute, *Petroleum Facts and Figures* (9th edn., New York, 1950), 444-45.

third share of the Mexican market controlled by Pierce, but negotiations broke down. In the price war that ensued, the Pearson interests emerged with 160 depots and installations throughout the country and 40 percent of the domestic market.⁴ For the next fifteen years Pierce had to share the market with Doheny's firm, Huasteca, and Pearson's firm, El Aguila. Pierce no longer imported U.S. products but, like his competitors, sold products refined from Mexican crude oil.

Before the outbreak of the First World War the international petroleum market began to influence the independent producing companies of Doheny and of Pearson (by then known as Lord Cowdray). Both independents struck a number of gushers in the Golden Lane, a series of oilfields running along the Gulf Coast between Tampico and Tuxpan and approximately three hundred miles south of the Texas border (see Map 1). The great well at Potrero del Llano, west of Tuxpan, came in, and El Aguila faced a burgeoning production that far exceeded the demands of the Mexican market. The company had neither foreign markets nor means to reach them. Its first exports consisted of selling crude oil on long-term contracts to international marketing giants like Standard Oil of New Jersey, which by this time had severed its relationship with Waters-Pierce.⁵ Doheny, who discovered the heavy oil deposits in the El Ebano-Pánuco area near Tampico, followed Cowdray into the southern fields, striking gushers at Casiano and Cerro Azul.⁶ He, too, sold crude oil to marketing companies.

Rising prices during the First World War enhanced the profits of the independent producers and permitted their vertical integration (uniting production, refining, and marketing within the same organization). Cowdray sought to create his own foreign marketing apparatus, establishing an office in New York to sell crude oil to American refiners, buying into a marketing company in Great Britain, and purchasing tanker transport. Before the war the Anglo-Mexican, Cowdray's international marketing firm, had completed construction of fuel oil installations at Rio de Janeiro and Buenos Aires, where Mexican petroleum began fueling railways and utilities.⁷ El Aguila's production required transport as well, and in 1919 Cowdray's transport affiliate had at least eight steam tankers and eleven more on order. By now El Aguila had three refineries operating in Mexico. Yet Cowdray bought (at a cost of £3 million) additional depots and bunkering stations in England and Europe.⁸ Greater volume meant greater need for vertical integration.

In a sense, petroleum was a nerve-racking business. The product was so great and expenses so high that failure to find stable, long-term markets often spelled quick bankruptcy. Consequently, in 1918, when his men first reported salt water intrusion into the southern fields, Cowdray suggested that production in other

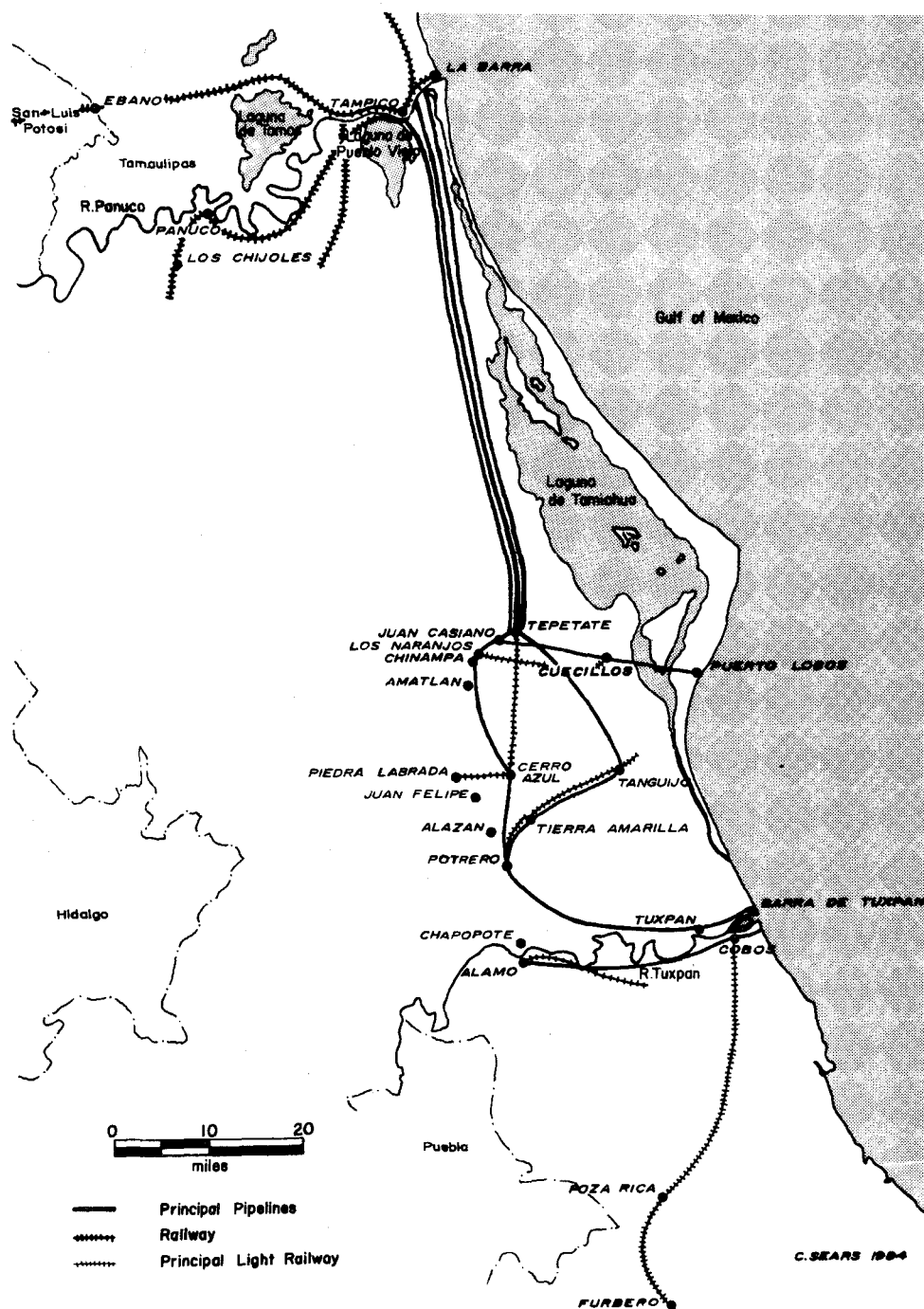
⁴ "Summary of Correspondence: Negotiations with WPO Co.," n.d., Science Museum Library, London, S. Pearson and Son, Ltd., and Associated Companies, Historical Records [hereafter, Pearson Papers], box C44, file 7.

⁵ Contract, April 4, 1912, *ibid.*, box C43, file 2.

⁶ Pan American Petroleum and Transport Company, *Mexican Petroleum* (New York, 1922), 29–35, 93. Pan American Petroleum was Doheny's holding company for all his producing and marketing firms.

⁷ Cowdray to Thomas J. Ryder, August 31, 1914, Pearson Papers, box A-3.

⁸ "Deed of Indemnity and Charge," April 1, 1935, *ibid.*, box C44, file 5; and Cowdray, "Memorandum re. the Aguila Co.," January 13, 1919, *ibid.*, box C44, file 3.



Map 1: Northern Vera Cruz Oil Zone, 1918.

SOURCE: Map entitled "Northern Vera Cruz Oil Area, 1918," University of Texas, Austin, Benson Latin American Collection, Map Archive.

wells be increased to make up for the declining flow of crude oil.⁹ Failure to fulfill sales contracts threatened the very existence of his growing oil organization. In addition, the war prevented Cowdray from reaping many advantages from the rise in prices during the period. Interest rates were high, the British Admiralty took over nearly one-half of his new tanker fleet, and shipyards halted production on other tankers. Meanwhile, the Admiralty obtained some fuel oil from El Aguila but covered most of its needs from sources in the United States.¹⁰ Cowdray felt vulnerable.

In fact, Cowdray all along had been seeking buyers to share the cost of expanding the marketing system or provide secure transport and markets. In 1912 he negotiated with the president of Jersey Standard, John Archbold, and in 1916 with Archbold's successor, Walter Teagle. Both thought that the asking price was too high. Moreover, the British government discouraged the sale of British oil interests to foreigners during wartime, whereupon Cowdray suggested that the government itself sponsor a national company. His Majesty's Government declined.¹¹ When the war ended Cowdray finally negotiated the sale of a minority interest to Royal Dutch/Shell, a Dutch and British company with extensive retail networks not only in Europe but also in the United States. As part of the purchase agreement, Shell assumed managerial control of the Mexican properties and of the transport and marketing apparatus in England and Latin America.¹²

Doheny's Huasteca experienced much the same growth toward vertical integration but without the wartime restrictions that hindered Cowdray. While selling crude oil to exporters through his own pipelines, Doheny completed a \$2 million refinery at Tampico with a capacity of one hundred thirty thousand barrels per day. Since Huasteca exported nearly six hundred thousand barrels per day in 1921, Doheny formed a number of international marketing companies with depots in Buenos Aires, Montevideo, and Rio de Janeiro, and in the American states along the Gulf of Mexico and Atlantic Coast. Subsidiaries operated refineries in New Orleans and Baltimore. He enlarged the fleet to a total of fifty tankers and oil barges.¹³ Expanding production and high profits enabled the foreign companies producing in Mexico to establish a presence that was nearly worldwide.

The United States was the most important customer for exported Mexican petroleum; its Gulf and Atlantic ports took approximately three-quarters of all Mexican oil exports. Primary uses of Mexican petroleum in the 1920s included bunker fuel for steam and diesel ships and fuel oils for homes and industries. The fuel oil market began to boom following the war, when manufacturers of oil

⁹ Cowdray to J. B. Body, May 13, 1918, *ibid.*, box A-3.

¹⁰ "Anglo-Mex's Proposals," May 19, 1916, *ibid.*, box C49, file 2; and Cowdray to Ryder, August 31, 1914; Body, "Memorandum," September 6, 1915, *ibid.*, box A-3.

¹¹ Geoffrey Jones, *The State and the Emergence of the British Oil Industry* (London, 1981), 190-91; and "Memo on Pearson's Oil Interests in Mexico," n.d., Public Record Office, London, Foreign Office Records [hereafter, FO] 371-2964/230821.

¹² "Letter of Undertaking: S. Pearson and Son, Ltd., to the Royal Dutch Company and the Shell Transport and Trading Company, Ltd.," March 26, 1919, Pearson Papers, box C44, file 3.

¹³ *Annual Reports of the Mexican Petroleum Company*, 1920, 1922, Huntington Library, San Marino, Calif.; Senate Committee on Foreign Relations, *Investigation of Mexican Affairs*, 230, 242-43; and *Pan American Record*, July 1917, pp. 3-8.



This automobile dealership and oil sales store in Aguascalientes (ca. 1920) illustrates the rise in domestic oil consumption in Mexico. Foreign oil companies met the demand with domestically refined Mexican oil. Photograph from the University of Texas, Austin, Miscellaneous Photograph Collection, Benson Latin American Collection, and reproduced courtesy of the General Libraries, University of Texas, Austin.

burners in New York had difficulty supplying those who wanted to convert from coal- to oil-burning installations. Most of the fuel oil came from Mexico's southern fields. By 1919, moreover, American refineries yearly were turning out six hundred seventy-five thousand tons of asphalt from Mexican crude oils of the Pánuco fields.¹⁴ Between 1908 and 1919 the petroleum producers of Mexico, dominated by the large independents, El Aguila and Huasteca, had captured not only the domestic market but also a large share of the U.S. market for heavy fuels and oils.

In the Golden Lane, the oil companies operated under a system of private contracts that permitted remarkably few contacts with Mexico's governing elites. Both Doheny and Cowdray acquired oil properties through leases from private landowners. Mexico's mining laws of 1884 and 1892, which had established the private contract, actually reversed centuries of Spanish legal custom.¹⁵ Colonial laws had vested mineral rights in the monarchy, a privilege that was assumed by the governments of most Latin American countries, but not Mexico, in the postcolonial period. Thus, foreign oil prospectors in Mexico obtained oil properties under

¹⁴ Harold F. Williamson *et al.*, *The American Petroleum Industry*, volume 2: *The Age of Energy, 1899-1959* (Evanston, Ill., 1963), 115; George P. Shaw, "Review and Summary of the Mexican Petroleum Situation in the Tampico Consular District for 1922," January 1, 1923, National Archives, Washington, D.C., General Records of the Department of State [hereafter, DS], Record Group [hereafter, RG] 812.6363/1358; and Robert Adamson to Senator William N. Calder, February 2, 1920, DS, RG 812.6363/642.

¹⁵ Rippy, *Oil and the Mexican Revolution*, chaps. 1-4; and José Colomo, *The Mexican Petroleum Law: Its Basis and Its Aims* (Mexico City, 1927), 8-10.

private contract and paid rental fees and royalties to Mexican landowners but little to the government. By 1919 approximately one hundred fifty different oil firms were operating in the Golden Lane—nearly all on private contracts.¹⁶

The governing elites in Mexico had little stake in the private oil industry, and, as production increased, the Mexican government began to overturn private contracts in petroleum production. Before the Mexican Revolution several supporters of President Díaz were members of the board of El Aguila.¹⁷ Beyond this, a few Mexican lawyers served the oilmen. Most companies were incorporated abroad, and their private contracts were made with landowners on the Gulf Coast who had little connection with the Díaz clique and who were not among the important land barons of the central and northern plateaus. The revolution, furthermore, brought to power a group representing the northern states that dominated the constitutional convention and inserted Article 27 into the Constitution of 1917. Resurrecting Spanish legal tradition, Article 27 declared subsoil resources to be national property—not to be alienated by private contract.¹⁸ President Venustiano Carranza, who effectively consolidated his power in 1916, was particularly active in increasing oil taxes and in attempting to rescind existing private contracts.

During the revolution the only group to support the private contract formed in the oil zone where General Manuel Peláez had his headquarters. The relationship between Peláez and the oil companies was a complicated one—not because Peláez received funds from the companies, nor because his family leased land to El Aguila, nor because he fought the Carranza nationalists. His guerrilla forces were a nuisance and a danger. The companies paid him “under duress” not to cut the pipelines, yet his protection was ineffective. When he proved unable to prevent Carranza’s forces from sacking the oil camps, oilmen concluded that Peláez’s surrender was desirable—to stop the fighting around flammable oil wells.¹⁹ That the oil boom continued in the midst of an armed struggle for control of the Golden Lane is indicative of the competitiveness of the oil companies in Mexico during the First World War. Once Peláez was neutralized in 1920, Mexico’s next presidents, Alvaro Obregón and Plutarco Elías Calles, again attempted to enforce Article 27 of the constitution. In this, they counted on the support of Mexican public servants, who had little stake in the oil industry. Mexico’s bureaucratic state was built in the late nineteenth century with revenues earned by export duties on agricultural and mineral production, a trade disrupted by the revolution. Because the government was bankrupt and their salaries unpaid, public servants supported higher taxes on the oil industry in the early 1920s—at the moment when oil exports were highest.²⁰ Public servants united with the governing politicians in Mexico against the private, foreign oil industry.

¹⁶ José Vázquez Schiaffino *et al.*, *Informes sobre la cuestión petrolera* (Mexico City, 1919), appendix (*anexo*).

¹⁷ Body to Pearson, January 8, January 31, 1906; Body to Cowdray, June 28, 1911, Pearson Papers, box A-4.

¹⁸ Katz, *Secret War in Mexico*, 256; and E. V. Niemeyer, Jr., *Revolution at Querétaro: The Mexican Constitutional Convention of 1916–1917* (Austin, Tex., 1974), 138–42.

¹⁹ Manuel Peláez, “To the Mexican People,” translation, December 31, 1917, Pearson Papers, box A-3; Stewart to Maurice De Bunsen, April 6, 1918, FO 371-3243/63332; Senate Committee on Foreign Relations, *Investigation of Mexican Affairs*, 281–85, 301–03, 553; and Peter S. Linder, “Every Region for Itself: The Manuel Peláez Movement, 1914–1923” (M.A. thesis, University of New Mexico, 1983).

²⁰ John Womack, Jr., “The Mexican Economy during the Revolution, 1910–1920,” *Marxist Perspectives*, 1

THE SHEER VELOCITY AT WHICH PRODUCTION and foreign markets expanded, meanwhile, encouraged the internationalization of the Mexican oil industry. When oil prices fell after World War I, the international companies Jersey Standard and Shell, seconded by U.S. companies, such as Gulf, Texas, Sinclair, and Standard Oil of Indiana, moved to absorb Mexico's independent producers. This process of consolidation proceeded even as the revolutionary government imposed on the foreign industry its political program of petroleum nationalism.

Jersey Standard began operations in Mexico in 1912, buying El Aguila and Huasteca crude oil and building a skimming plant on the Pánuco River near Tampico. Soon it became evident that Jersey had to acquire its own production or risk dependency on crude oil bought from competitors. Jersey prospectors arrived in Mexico's Golden Lane and secured leases near the Huasteca and El Aguila wells in the southern fields.²¹ After attempts to purchase El Aguila failed, Jersey Standard bought (for \$2.5 million) Transcontinental, a small producing company, three months after promulgation of the 1917 constitution. Jersey Standard enjoyed a rare opportunity for expansion in wartime, when the independent producers were unable to find shipping for their crude oil and few could afford to borrow at current high interest rates.²² The company set about extending Transcontinental's pipelines, drilling new wells, and building storage and loading facilities on the Gulf Coast. It acquired the La Barra refinery near Tampico as well as additional oil leases. By 1922 the dollar investment in Transcontinental had increased to more than \$32.5 million.²³

The Royal Dutch/Shell group, which had extensive marketing operations in Europe and the United States, accompanied Jersey Standard into Mexico. Arriving in the midst of the oil boom, Dutch geologists competed with U.S. prospectors for leases and contracts from landholders and speculators. Shell formed La Corona late in 1912 to work the twenty thousand acres of leases in the heavy oil district of Pánuco and in 1919 bought a minority interest—but with management control—in the El Aguila organization, which moved Shell to the forefront of the Mexican oil industry.²⁴ El Aguila's refinery at Tampico maintained a throughput of approximately eighty-two thousand five hundred barrels per day, while those of Tuxpan and Minatitlán averaged fifteen thousand and ten thousand barrels per day

(1978): 103; Ramón Eduardo Ruiz, *The Great Rebellion: Mexico, 1907–1924* (New York, 1980), 50–54; John W. F. Dulles, *Yesterday in Mexico: A Chronicle of the Revolution, 1919–1936* (Austin, Tex., 1972), 106, 330; and N. Stephen Kane, "Bankers and Diplomats: The Diplomacy of the Dollar in Mexico, 1921–1924," *Business History Review*, 47 (1973): 334–52.

²¹ C. O. Swain to Frank Wilson, December 3, 1913; Swain to E. Arredondo, May 7, 1915; Swain to C. T. White, March 26, 1915; and "List of Leases Protocolized in Name of John Kee," April 19, 1917, Standard Oil Company (New Jersey), New York, Esso Standard Records [hereafter, ESR], Legal Department, file 117. The Esso Standard records and the records of Standard Oil Company (New Jersey), collected by Henrietta M. Larson, apparently no longer exist.

²² E. J. Sadler to S. B. Hunt, February 18, 1918, Standard Oil Company (New Jersey), New York, Standard Oil Company (New Jersey) Records [hereafter, SONJ], Directors Files, E. J. Sadler Papers; and SONJ, Production Department, "Transcontinental Contract Files," August 24, 1917.

²³ SONJ, Controller's Department, Reports of Transcontinental, 1918–28; and George Sweet Gibb and Evelyn H. Knowlton, *The Resurgent Years, 1911–1927: History of Standard Oil Company (New Jersey)* (New York, 1956), 84–89.

²⁴ F. C. Gerretson, *History of the Royal Dutch*, 4 vols. (Leiden, 1953), 4: 264–65.

respectively. In 1921 the company's export trade earned more than 7.3 million pounds in Europe and South America and an additional 2.4 million pounds in trade with the United States. Its domestic sales in the Mexican marketplace amounted to more than 62 million pesos (approximately 10 million pounds).²⁵ Unlike Jersey Standard, the Shell group had acquired a share of Mexico's domestic market.

The process of consolidation had no sooner begun than Mexico began to lose its competitiveness in the international marketplace. At a time when world production was rising and prices for crude oil were falling, salt water cut production in the wells of the Golden Lane. The effluent was most evident in the southern fields near Tuxpan, from which had come the more valuable light Mexican crude oil (see Table 2). El Aguila's great well at Potrero del Llano, which had produced more than ninety million barrels of crude, was affected in 1918.²⁶ In some areas the predominance of small leaseholds accentuated the salt water problem: competing companies with drilling rights on properties situated over common reservoirs were encouraged to pump at the fastest possible rate in their race to exploit the reservoir before it was exhausted by the flow of salt water.²⁷ The "offset campaign" and salt water intrusion, both of which promised early exhaustion of the wells, motivated a decidedly short-term strategy on the part of the producers. As early as March 1920, Jersey Standard president Walter Teagle wrote:

Frankly, I am much concerned in regard to the Mexican situation, and especially as to whether we will ever be able to get this large investment back. As I have understood it, Messrs. Sadler and Corwin [of the foreign producing department] have always expressed themselves as feeling that we had this year and next to get our money back from Mexico—that the production will be there for at least this length of time. I sincerely trust that such will prove to be the case, for if it does not, I do not see how we will ever get the money back.²⁸

Companies that worked the southern fields exclusively were hit hard by salt water intrusion—for example, Jersey Standard, whose ability to supply its fuel oil contracts was jeopardized. As early as 1921, Transcontinental, Jersey's subsidiary, began to sever its obligations to customers along the East Coast of the United States.²⁹ Although Jersey opened up new production in heavier crude oil at Pánuco, its stocks of all Mexican crudes dwindled drastically after 1926 (see Table 3). Ironically, the company's intransigence increased. Jersey protested the Mexican petroleum law of 1925, which required the exchange of private property rights for government concessions, and disapproved of the Calles-Morrow accord of 1928, which recognized the state's authority over oil resources. Jersey Standard reasoned that any renegotiation of what it considered inalienable property rights in Mexico

²⁵ Mexican Eagle Accounts, Pearson Papers, box C44, file 10.

²⁶ James D. McLachlan, "Report upon the Situation in Mexico," December 17, 1918, FO 371-3826/472.

²⁷ Interview with L. Philo Maier (former Jersey engineer in Mexico in 1920), Coral Gables, Fla., December 30, 1982; *Carta de la zona petrolífera del Norte de Vera Cruz*, March 1919, University of Wisconsin, Milwaukee, American Geological Institute Map Collection; Swain to secretary of state, August 17, 1921, DS, RG 812.6363/923; and Hickerson to secretary of state, August 20, 1921, DS, RG 812.6363/936.

²⁸ Teagle to Sadler *et al.*, March 2, 1920, SONJ, Sadler's Old Mexican Files.

²⁹ Sadler to H. Reidemann, September 19, 1921, *ibid.*; and J. A. Brown to Swain, ca. December 1919; Swain to Henry P. Fletcher, December 9, 1919, ESR, Legal Department, file 117.

TABLE 2
Production in the Pánuco and Southern Fields of Mexico, 1914–25
(in barrels)

<i>Year</i>	<i>Pánuco</i>	<i>Southern</i>
1914	7,076,544	19,158,859
1915	3,848,398	29,062,010
1916	12,088,397	28,458,518
1917	20,502,300	34,790,470
1918	19,857,361	43,970,965
1919	31,836,581	55,236,373
1920	42,880,275	120,158,708
1921	44,641,849	158,120,678
1922	46,538,664	137,881,021
1923	88,352,774	63,374,796
1924	100,493,754	40,119,101
1925	78,081,983	37,702,964

SOURCES: Charles A. Bay, "Review of the Petroleum Industry in Tampico, 1924," National Archives, Washington, D.C., General Records of the Department of State, Record Group 812.6363/1558, and "Review of the Petroleum Industry in Tampico, 1925," National Archives, Washington, D.C., General Records of the Department of State, Record Group 812.6363/1760.

TABLE 3
The Mexico Oil Operations of Transcontinental,
Subsidiary of Standard Oil Company (New Jersey), 1918–30
(daily averages in barrels)

<i>Year</i>	<i>Net Production</i>	<i>Crude Oil Purchases</i>	<i>Crude Oil Sales</i>
1918	2,552	284	958
1919	21,236	2,360	9,475
1920	38,640	36,034	5,834
1921	36,499	22,573	210
1922	13,236	26,439	1,563
1923	58,713	9,586	19,253
1924	50,234	37,473	1,318
1925	45,570	42,145	6,263
1926	19,079	30,762	2,863
1927	9,784	15,846	2,238
1928	6,369	12,092	2,050
1929	5,048	9,763	2,449
1930	4,542	6,760	1,763

SOURCE: Standard Oil Company (New Jersey), New York, Standard Oil Company (New Jersey) Records, Coordination and Economics Department, Statistical Tables Compiled by the Department. Documents in the author's possession, see note 21, above.

would prejudice its future position in the really important production of Venezuela, where the company had begun the transfer of machinery and pipelines.³⁰ In 1928 Transcontinental closed its refinery at Tampico. Finally, the company signaled its virtual withdrawal from Mexico by turning over all of its production and transportation facilities to Huasteca.³¹ In view of the eroding competitiveness of Mexican crude oil, Jersey Standard, which had never established for itself a share in the domestic market, had little reason to remain in Mexico.

Because of its commanding position in the southern fields, El Aguila's decline was the most spectacular (see Table 4). In order to meet its sales contracts, the Shell company had to purchase crude oil, which presented another problem. Falling production and increasing taxes had pushed upward local prices for Mexican crude. El Aguila refiners at Tampico reported that the cost of producing Pánuco oil exceeded the market price, because not enough products could be extracted from the heavy crude oil to make it pay. The failure of the southern fields forced El Aguila to purchase crude from smaller companies in Mexico and to experiment with the importation of Californian and even Venezuelan crude oil, which at Tampico cost a few pennies per barrel less than did Pánuco oil. Huasteca also had to buy crude from other companies in order to fulfill its contracts.³² Having anticipated the oil exhaustion in Mexico, Doheny had invested in a Venezuelan company, Lago, which began supplying some United States markets in the mid-1920s. In the meantime, Shell's large refinery near Rotterdam, which had converted Mexico's heavy crude into road asphalt, adjusted the process in order to use Venezulean crude instead.³³ Mexico's decline stimulated production in Venezuela.

The local price rise eroded Mexico's comparative advantage in the 1920s. Prices in the United States, a principal market for the Mexican product, were in fact falling as a result of oil discoveries in Texas, Louisiana, Oklahoma, and California (see Table 5). The flow of oil from Mexico to the United States was, in part, reversed. For example, at Guadalajara, where gasoline prices rose to the equivalent of thirty-seven cents per gallon, the chamber of commerce began to buy gasoline in tankcars at Laredo and transport it to the city for sales at twenty-five cents per gallon. Finally, Standard Oil of California in 1925 opened up depots from Sonora to Mexico City that distributed Californian oil products. Mexican consumers on the West Coast welcomed the imports because they were cheaper than domestic petroleum.³⁴ After fifteen years imported oil had returned to the Mexican

³⁰ See Jonathan C. Brown, "Jersey Standard and the Politics of Latin American Oil Production, 1911-1930," in John D. Wirth, ed., *Latin American Oil Companies and the Politics of Energy* (Lincoln, Neb., forthcoming); and Philip, *Oil and Politics in Latin America*, chap. 10.

³¹ Sadler to Arthur C. Corwin, June 25, 1925, SONJ, Production Department, Sadler's Old Mexican Files; Peter H. A. Flood, "Review of the Petroleum Industry in the Tampico District during the Year 1927," March 30, 1928, DS, RG 812.6363/2592; and Robert Harnden, "Changes in Administration of the Petroleum Companies," July 25, 1929, DS, RG 812.6363/2655.

³² J. Assheton to Body, March 19, 1925, October 27, 1924; Body to Assheton, January 7, 1925; Assheton to Body, August 3, 1925, December 31, 1926, Shell International Petroleum Company, London, Group History, Country Series, Mexico Management [hereafter, Shell GHC/MEX], files D29/1/1, D29/1/2, D29/2/2; and Flood, "Review of the Petroleum Industry."

³³ Assheton to Body, August 3, 1925, Shell GHC/MEX, D29/1/2; and B. W. Smith, "The Story of Royal Dutch/Shell up to 1945" (unpublished MS, Shell International Petroleum Company, London, 1961), 166.

³⁴ Dudley G. Dwyer, "Refined Products, Gasoline," December 6, 1924, DS, RG 812.6363/1542; and J. R. Douglas, "California Standard Oil Co. de México," August 12, 1929, DS, RG 812.6363/2659½.

TABLE 4
The Mexico Oil Operations of El Aguila,
Affiliate of Shell International Petroleum Company, 1921-32
(daily averages in barrels)

<i>Year</i>	<i>Production</i>	<i>Oil Purchases</i>	<i>Total Shipments^a</i>
1921	134,928	11,792	--
1922	33,946	14,846	61,970
1923	25,347	30,411	59,203
1924	14,913	37,824	47,844
1925	18,751	23,594	41,164
1926	23,301	17,781	44,795
1927	--	--	--
1928	16,657	21,280	36,878
1929	32,751 ^b	18,536	40,698
1930	35,124	15,217	51,735
1931	33,145	11,933	44,726
1932	37,090	12,396	45,906

^a Total shipments include exports as well as products sold in Mexico.

^b El Aguila absorbed La Corona in 1929.

SOURCE: Science Museum Library, London, S. Pearson and Son, Ltd., and Associated Companies, Historical Records, box C43, file 5, Mexican Eagle Oil Company, Reports of the Directors and Accounts.

marketplace. As a consequence, several companies in Mexico followed Jersey Standard's example and reduced production. Yet those with the largest stakes in Mexico remained there. According to an estimate of 1933, oil companies had constructed 1,436 oil storage tanks at refineries and sales depots throughout the country and approximately fifteen hundred kilometers of pipelines.³⁵ Although their original investment had long since been amortized, the companies did not want to abandon these expensive installations. No doubt the oilmen expected that international oil prices would eventually rise.

Another motive for remaining was the increasing importance of Mexico as a domestic market in the late 1920s. As its production declined, El Aguila's domestic sales grew, a reflection of Mexico's postrevolutionary recovery. From 1926 to 1927 the company's gasoline sales in Mexico rose 12.9 percent, while its sales of kerosene, grease, and wax increased by 5.7, 9.5, and 48.7 percent respectively. El Aguila's gross sales receipts from domestic marketing amounted to \$14 million, a figure that surpassed its export receipts.³⁶ In 1930 the oil companies expressed confidence in Mexico's market (if not in its production). Sinclair bought out Pierce, successfully connecting Mexican production to Mexican marketing. Soon thereafter, El Aguila

³⁵ Jesús Silva Herzog, *El petróleo de México* (Mexico City, 1940), 47.

³⁶ Assheton to Body, August 31, 1926, Shell GHC/MEX, D29/2/1; and "Results for Year 1932 and Comparison with Prior Years," Pearson Papers, box C43, file 7.

completed construction of its refinery in Mexico City and pumped three thousand barrels per day of crude oil to it through its new one-hundred-forty-mile pipeline from Poza Rica.³⁷ At the time, Mexico was second only to Argentina as a market for oil sales in Latin America and consumed petroleum products at twice the rate of Brazil, which was third. In 1933 El Aguila led all suppliers with sales of 41,038,451 liters, followed by Huasteca with 29,503,826, Pierce with 26,320,950, and California Standard with 24,832,944.³⁸ Foreign petroleum provided fully one-fifth of the Mexican market for gasoline.

The importance of the domestic market in part persuaded Jersey Standard to return to Mexico. Jersey bought from Standard Indiana not only Doheny's old oil fields and refinery at Tampico but also his U.S. and Latin American marketing organization and the Lago company in Venezuela along with its new refinery at Aruba.³⁹ Superficially, this seems to have been a great vote of confidence in Mexico's oil future by a company whose executives had criticized the petroleum law of 1925 and the Calles-Morrow agreement of 1928. Clearly, the Venezuelan portion of the purchase was paramount. Yet Jersey Standard now established for the first time a presence in the retail petroleum business of Mexico by acquiring, in 1932, Huasteca, which controlled nearly a quarter of the Mexican marketplace.

THE DEVELOPMENT OF THE VENEZUELAN OIL INDUSTRY awaited Mexico's decline. The first concessions for petroleum were extended early in the twentieth century to Venezuelans, who in turn sought foreign capitalists to develop the suspected resources. The few New York and London investors who purchased interests in Venezuelan concessions accomplished little, for there were no markets. Venezuelan domestic consumption of petroleum products was meager, since the country had none of the railways, mining, and urbanization that made Mexico, Argentina, and Brazil so attractive. A sales affiliate of Jersey Standard, the West Indian Oil Company, maintained in Caracas a small staff that imported U.S. kerosene and lubricants. Moreover, not many foreign markets existed. Once Mexico had captured the United States market for heavy oils, few independents seemed interested in the more isolated and as yet undiscovered Venezuelan resources that were expensive to develop.

Unable to build an industry in Venezuela as Doheny and Cowdray had in Mexico, the independent oilmen left that task to the major international companies, led by the Royal Dutch/Shell group. Even so, a company with the obvious capital and marketing advantages of Shell could not organize the Venezuelan industry rapidly. Shell entered Venezuela in 1912 but did not export in bulk for eleven years. Sir Henri Deterding had become interested in Venezuelan oil because he

³⁷ Pearson Papers, box C43, file 5, Mexican Eagle Oil Company, Reports of the Directors and Accounts, 1933.

³⁸ Silva Herzog, *El petróleo de México*, 46; and Philip, *Oil and Politics in Latin America*, 80.

³⁹ Paul H. Giddens, *Standard Oil Company (Indiana): Oil Pioneer of the Middle West* (New York, 1955), 489-93; and Mira Wilkins, *The Maturing of Multinational Enterprise: American Business Abroad from 1914 to 1970* (Cambridge, Mass., 1974), 210.

TABLE 5
Average U.S. Price per Barrel of Crude Oil at the Well, 1900–34
(in U.S. dollars)

1900–1916	Price per Barrel	1917–1934	Price per Barrel
1900	\$1.07	1917	\$1.56
1901	.72	1918	1.98
1902	.69	1919	2.21
1903	.89	1920	3.08
1904	.86	1921	1.73
1905	.65	1922	1.61
1906	.73	1923	1.34
1907	.72	1924	1.43
1908	.72	1925	1.68
1909	.70	1926	1.88
1910	.61	1927	1.30
1911	.61	1928	1.17
1912	.74	1929	1.27
1913	.95	1930	1.19
1914	.81	1931	.65
1915	.64	1932	.87
1916	1.10	1933	.67
		1934	1.00

SOURCES: Harold F. Williamson *et al.*, *The American Petroleum Industry*, volume 2: *The Age of Energy, 1899–1959* (Evanston, Ill., 1963), 39, 464, 539; and American Petroleum Institute, *Petroleum Facts and Figures* (9th edn., New York, 1950), 170.

wanted to build up Shell's marketing apparatus in South America, hitherto practically a Jersey Standard monopoly. He hired a team of forty geologists, led by Ralph Arnold, who selected forty drilling sites—mostly in the Maracaibo Basin of western Venezuela. The first well at Perija struck oil in February of 1914. The European war stopped further drilling and the construction of pipelines, storage, and transport facilities—much to the chagrin of a Venezuelan government eager for petroleum revenues. In 1918 Shell brought on line a small refinery at San Lorenzo, which undermined the import business of Jersey Standard.⁴⁰ Because of its operating philosophy, Shell was unable to develop the Venezuelan properties as fast as the Venezuelan president, Juan Vicente Gómez, and his government desired. Deterding subscribed to the "straight-line" policy: a successful international company should sell only in markets close to the oil fields. In 1920 Royal Dutch/Shell was still in the process of building markets in Latin America.⁴¹ It had acquired

⁴⁰ Willis C. Cook, "Petroleum Industry in Venezuela, 1921," May 19, 1922, DS, RG 831.6363/98; Memorandum, February 15, 1918, DS, RG 831.6363/17; Gerretson, *History of the Royal Dutch*, 3: 55, 4: 281; and Ralph Arnold *et al.*, *The First Big Oil Hunt: Venezuela, 1911–1916* (New York, 1960), 63, 91.

⁴¹ Henri Deterding, *An International Oilman* (London, 1934), 50–51; and Wilkins, "Multinational Oil Companies in South America," 417–18.

Cowdray's sales network in the region by purchasing El Aguila, but until about 1923 Mexico's production exceeded all the demands of Shell's market apparatus. Then in 1922 Shell brought in Venezuela's first gusher in the La Rosa field, and the oil rush was on (see Map 2).

Jersey Standard's interest in Venezuela was a response to initiatives made by Shell. A team of Jersey geologists had surveyed the Venezuelan oil scene as early as 1915 but declined to purchase a large, undeveloped concession.⁴² Four years later a second geologist reported that Venezuela was unattractive except as a reserve area in case of a shutdown in Mexico.⁴³ At the time, Jersey Standard had committed itself to a strong program of expansion in Mexico. Nevertheless, its officials concluded that, if Shell was investing millions there, Venezuela must have oil. In 1919 Jersey Standard began to acquire Venezuelan territories for exploration. Like others, this company preferred to purchase existing concessions because the terms were more agreeable. Concessions granted before the 1922 petroleum law were larger, could be held for longer periods, and were taxed by the government at lower rates. Subsequent petroleum laws were more restrictive.⁴⁴ Therefore, Jersey Standard bought one-half interest in the 1915 Buchivacoa concession from British Controlled Oilfields and ultimately secured some 1.2 million hectares of exploratory concessions through purchases from other companies and from Venezuelan middlemen such as Julio Méndez, the president's son-in-law.⁴⁵ Dealing with larger concessions rather than small private contracts enabled oilmen in Venezuela to avoid the offset competition that hastened the exhaustion of Mexico's oilfields.

Seven years passed as Jersey Standard drillers tramped through Venezuela's wilderness, spudding in one dry well after another. Standard Oil of Venezuela, a subsidiary, spent \$40 million before exporting its first barrel of oil. Reporting on the frustrating years of exploratory drilling, E. J. Sadler of the foreign producing department wrote:

Up to the present time, the Standard Oil Company has drilled about forty wildcat wells in Venezuela, mostly on surface structures or close to seepage. So far, no well has been completed which has made a commercial producer. . . . We have no particularly desirable localities at the present time on which to suggest drilling.⁴⁶

Jersey Standard began to look toward the purchase of producing companies at about the time that its Mexican production fell definitively. In 1926 Teagle of Jersey Standard conferred with Deterding of Shell about the possible purchase of the Colon Development Company, a large concession holder and producer in Venezuela. Teagle resisted the investment, reasoning that Deterding would sell

⁴² "Venezuela: Caribbean Petroleum Co.," September 1, 1915, SONJ, Production Department, E. J. Sadler Papers.

⁴³ M. M. Thompson to Corwin, January 16, 1919; Thompson, report, April 18, 1919, *ibid.*

⁴⁴ C. Van H. Engert to secretary of state, May 18, 1929, DS, RG 831.6363/Creole I; and Rutherford Bingham to William Warfield, September 28, 1922, Standard Oil Company (New Jersey), New York, Creole Petroleum Company Records [hereafter, Creole Records].

⁴⁵ "Concessions—Méndez"; "Concessions—Sanabria," Creole Records; and McBeth, *Juan Vicente Gómez and the Oil Companies*, 108.

⁴⁶ "Annual Report of Standard Oil Company of Venezuela, 1927," SONJ, Controller's Department; Wayne C. Taylor *et al.*, *The Creole Petroleum Corporation in Venezuela* (Washington, D.C., 1955), 13; and Sadler memorandum, Maracaibo, June 19, 1926, Creole Records, Reports on Venezuela.



Map 2: Lake Maracaibo Oil Region, 1928.

SOURCE: Map published in *The Oil Weekly*, December 13, 1929, p. 90.

nothing of value.⁴⁷ Instead, Jersey Standard became interested in an American company, the Creole Syndicate, that held a concession adjacent to and under Lake Maracaibo. Creole, however, was not an operating company, its drilling having been contracted to a Gulf subsidiary that had struck oil in 1925. With production but without transport or markets, Creole stockholders voted in June 1928 to sell a majority interest to Jersey Standard, which now possessed a source of Venezuelan crude oil from which fuel oil and gasoline could be refined.⁴⁸ In the meantime, Jersey's drilling program had finally brought in a new field in western Venezuela that produced heavy oils for asphalt markets.⁴⁹

Obviously, Jersey Standard's expansion in Venezuela was not entirely a reaction to Mexican nationalism. First, it entered the country because its greatest competitor, Shell, was investing heavily in exploration and development there; second, Jersey sought a replacement for its declining production in Mexico (see Table 2). Business logic brought Jersey Standard to Venezuela.

The U.S. companies that entered Venezuelan production in the boom years from 1922 to 1929 did so at a disadvantage vis-à-vis British rivals. Latecomers had to pay higher surface taxes and royalties than did the Shell companies that arrived before the war.⁵⁰ Nonetheless, the Shell companies could not maintain their early hegemony for lack of markets, one of the reasons why Deterding considered the sale of the Colon. Although the State Department early in the 1920s sought to promote U.S. over British business interests, oilmen from the two countries cooperated to mutual advantage in Venezuela. British investors without sufficient funds to develop the concessions they acquired willingly sold a majority interest in them to U.S. companies and were content to share in the royalties, letting the Americans take over the management and the capital expense. In the fields, U.S. and British companies shared tools, pipelines, and terminal facilities.⁵¹

Unlike Mexico, Venezuela preserved the system of patrimonial concessions in mining and drilling. The concessions were usually acquired by Venezuelan middlemen, who then sought out oilmen and sold the contracts at a profit. Occasionally, the Venezuelans were quite aggressive in seeking buyers—one asked the U.S. Geological Survey to put him in touch with U.S. oil companies. The Venezuelan owners of the oldest petroleum operation in the country, the *Compañía Petrolía de Táchira*, founded in 1878, sent a circular letter to the Caracas offices of the foreign companies offering its concession for sale.⁵²

Like the ruling elites, government officials had a large stake in the private oil industry, which perhaps explains the broad support in Venezuela for foreign

⁴⁷ "Colon Development Company, Ltd."; Teagle to Sadler, April 1, 1926, Creole Records, Reports on Venezuela.

⁴⁸ Henrietta M. Larson *et al.*, *New Horizons, 1927–1950: History of Standard Oil Company (New Jersey)* (New York, 1971), 41–42, 58.

⁴⁹ Aníbal R. Martínez, *Chronology of Venezuela Oil* (London, 1969), 57; *The Oil Weekly*, June 20, 1930, p. 67; and Gibb and Knowlton, *Resurgent Years*, 384–91.

⁵⁰ Edwin Lieuwen, *Petroleum in Venezuela: A History* (Berkeley and Los Angeles, 1954), 33–35.

⁵¹ Willis C. Cook to secretary of state, June 8, 1922, DS, RG 831.6363/103; Dudley G. Dwyer, "Development of Petroleum in Venezuela," November 22, 1921, DS, RG 831.6363/48; and Joseph S. Tulchin, *The Aftermath of War: World War I and U.S. Policy toward Latin America* (New York, 1971), 152–53.

⁵² Joaquín González to George Otis Smith, Caracas, April 29, 1922, DS, RG 831.6363/105; and Cook, "Petroleum Industry in Venezuela, 1921."

investment. Oil export revenues surpassed all other sources of income in 1926, which made the newly expanding bureaucracy dependent on the petroleum industry.⁵³ By 1928, one hundred fifty petroleum companies had registered with the Venezuelan government; U.S. investments rose from \$75 million in 1924 to \$161.6 million in 1929.⁵⁴ The latter figure represented oil investments almost exclusively, and production responded to investments. Before oil prices broke completely in 1930, the Venezuelan industry was competitive. Wells and pipelines were in place, storage facilities and terminals constructed, and barge transport provided to carry the crude oil out of Lake Maracaibo to tanker ports and refineries at Aruba and Curaçao. The first marine derricks had even drilled wells into the lake bed.⁵⁵ Venezuelan production had surpassed that of Mexico.

Consolidation of the oil industry in Venezuela followed much the same pattern in the late 1920s and early 1930s as it had earlier in Mexico. Independent producers without sales outlets sold out to the marketing giants in a decade of falling prices. The British reduced their activity first. By 1922 most capital-short independent British firms had begun selling controlling shares of their concessions. Large but vulnerable U.S. independents soon followed. Doheny, who had developed a one-quarter share of the Mexican market but an inadequate portion of petroleum sales in the eastern United States, sold his Venezuelan Lago company along with Huasteca of Mexico to Standard Oil Company of Indiana, the marketing giant of the Midwest.⁵⁶ The pace of consolidation increased after 1930, when fuel prices suddenly dropped. The oil glut reached such proportions that Shell, Gulf, and Jersey Standard agreed to cut production in Venezuela by some 40 percent. Together, these companies convinced Indiana Standard to cut back its production during the following years.⁵⁷ Soon even the largest companies, especially those that lacked overseas markets, began to sell properties. Gulf delivered one-half of its interest in the Mene Grande field to Jersey Standard, which in turn sold one-quarter to Shell. Jersey Standard in 1932 further expanded the operations of its Creole affiliate through the purchase of Indiana Standard's Lago oilfields.⁵⁸

Venezuelan oil growth was remarkable because it was accomplished in a period of falling crude oil prices during the 1920s (see Table 5). In contrast, Mexico's boom of the previous decade had benefited from buoyant international demand. In order to remain competitive in the new era of low prices and expanding world production, both the Venezuelan and Mexican oil industries had to maintain active and efficient drilling programs. In this the companies in Mexico failed; those in Venezuela succeeded.

⁵³ McBeth, *Juan Vicente Gómez and the Oil Companies*, 111; and Jorge Salazar-Carrillo, *Oil in the Economic Development of Venezuela* (New York, 1976), 41.

⁵⁴ H. M. Wolcott, "The Venezuelan Petroleum Industry," December 22, 1928, DS, RG 831.6363/415; and "Latin American Oil Resources," *The Oil Weekly*, April 11, 1930, p. 76.

⁵⁵ See the special South American issue of *The Oil Weekly*, December 13, 1929.

⁵⁶ Giddens, *Standard Oil Company (Indiana)*, 240-42.

⁵⁷ Roy Leigh, "Maracaibo Restriction Agreement May Be Extended during 1930," *The Oil Weekly*, December 27, 1929, p. 23; and Giddens, *Standard Oil Company (Indiana)*, 462.

⁵⁸ Giddens, *Standard Oil Company (Indiana)*, 489-93; Edith Penrose, *International Petroleum Industry* (London, 1968), 117-18; and Larson *et al.*, *New Horizons*, 47-49.

POLITICAL COOPERATION PROVED ESSENTIAL to oilmen in the critical stage of exploration in both Mexico and Venezuela. In Mexico, Díaz encouraged Doheny and Pearson (Cowdray); in Venezuela, Gómez often assured foreign oilmen that he would do nothing to inhibit investments. Although he raised taxes on new concessions, Gómez never permitted a nationalistic attack on existing contracts. (Later nationalists accused him of *vende patria*, selling the fatherland.) He urged U.S. oilmen to compete with the Shell companies in order to accelerate petroleum exports.⁵⁹ British and U.S. geologists were permitted to cross and recross the hinterlands, and, by 1928, eighty-five drilling rigs were engaged in extensive wildcat operations in eastern and western Venezuela.⁶⁰

Although drilling in Venezuela was quite productive, the same could not be said of Mexico. Drilling there continued at a high rate, averaging some five hundred new wells per year between 1921 and 1928, but the results were increasingly unsatisfactory (see Table 6). Existing facilities made drilling cheaper on the proven oilfields of the Golden Lane. Wildcatting, on the other hand, was expensive. An exploratory well in Tabasco in the 1920s cost \$1.5 million, whereas an additional well in the oilfields near Tuxpan cost only \$100,000.⁶¹ Most important, Mexican drilling did not expand production because wells drilled in the old fields merely diminished the output of existing wells. Compared to the new fields being opened in Venezuela in 1928, well drilling in Mexico was uncompetitive. Of the 237 wells drilled in Mexico, only 96 were productive, each averaging 268 barrels per day, whereas 100 of 101 new wells drilled in Venezuela each averaged 1,376 barrels per day.⁶²

Simultaneously, Mexico's revolutionary nationalism increased the cost of existing production. President Obregón's production tax of 1921, in particular, increased the government's per-barrel revenues by 40 percent—despite a brief export boycott by U.S. oil companies.⁶³ Mexican labor strikes of the 1920s led to wage increases but added little to oil costs. When the construction of oil facilities approached completion, oilmen reduced their payrolls and switched to contract labor. Transcontinental cut the number of its permanent workers from 3,313 in 1920 to 931 in 1922, El Aguila from 8,300 in 1921 to 851 in 1924.⁶⁴ In Venezuela, meanwhile, the oil laws of 1922 and 1928 also increased governmental revenues from *new*

⁵⁹ Although Gómez involved the oilmen in the drafting of his petroleum laws, he succeeded in reducing the size of concessions and in increasing the taxation. Preston McGoodwin to secretary of state, July 23, 1921, DS, RG 831.6363/70; McBeth, *Juan Vicente Gómez and the Oil Companies*, chap. 2; Wilkins, *Maturing of Multinational Enterprise*, 114–45; and Betancourt, *Venezuela: Oil and Politics*, chap. 1.

⁶⁰ Alexander K. Sloan, "Structure Wells in the Maracaibo Lake Basin," November 15, 1926, DS, RG 831.6363/335; E. L. Hopkins, "Some Economic Notes on the Venezuelan Oil Developments," March 29, 1929, DS, RG 831.6363/419; and *The Oil Weekly*, December 13, 1929, p. 126.

⁶¹ A. R. Coleman to H. N. Branch, September 22, 1928, DS, RG 812.6363/2662; and Silva Herzog, *El petróleo de México*, 38.

⁶² Coleman to Branch, September 22, 1928, DS, RG 812.6363/2662.

⁶³ James Sheffield to secretary of state, June 29, 1926, DS, RG 812.6363/1890; Meyer, *Mexico and the United States*, 16; and Philip, *Oil and Politics*, 17.

⁶⁴ Assheton to Body, August 24, 1924, Shell GHC/MEX, D29/1/1; and SONJ, Controller's Department, Records of Transcontinental, 1918–28.

TABLE 6
Drilling Program of the Foreign Oil Companies in Mexico, 1921–29

<i>Year</i>	<i>Number of Wells Drilled</i>	<i>Number of New Wells Producing</i>
1921	334	279
1922	279	113
1923	471	217
1924	732	259
1925	796	241
1926	853	257
1927	544	194
1928	237	96
1929	111	32

NOTE: The figures for 1929 include only the first six months of the year.

SOURCES: Peter H. A. Flood, "Review of the Petroleum Industry in the Tampico District during the Year 1927," National Archives, Washington, D.C., General Records of the Department of State, Record Group 812.6363/2592; and Harold B. Minor, "Petroleum Industry in Tampico during June 1929," National Archives, Washington, D.C., General Records of the Department of State, Record Group 812.6363/2656.

concessions, but, since most oilmen were working cheaper, pre-1922 concessions, the cost impact of the laws was much delayed.

The oil companies sought actively to expand into new territories in the early 1920s—but Mexican politics intervened. U.S. geologists had been to the coastal areas in Chiapas and Tabasco as early as 1919, pronouncing the geological structures very promising. Several Jersey Standard men had surveyed the southern Gulf Coast, finding oil seepages below the Isthmus of Tehuantepec. Returning to Mexico City, however, they were unable to register for concessions on agreeable terms. El Aguila's exploration at Macuspana and Sarlat (today parts of Mexico's La Reforma fields) was prevented by Tomás Garrido, Tabasco's governor and labor leader.⁶⁵

The concessionary status of new exploration under the Constitution of 1917 was not an issue for the oil companies, which agreed in principle to accept concessions in Mexico as they did in Venezuela. But the government used this power as a lever both to pry away pre-1917 private property rights enjoyed by the companies in the Golden Lane and to raise taxes. In 1922, U.S. oilmen proposed formation of a petroleum development corporation of Mexico, in which the government and the foreign companies would cooperate in the exploration of seven hundred thousand hectares of new land *outside* the Golden Lane. The government insisted that all private oil properties, including the pre-1917 Golden Lane holdings, be incorporated.⁶⁶

⁶⁵ Kyle Kinney to American consul, Vera Cruz, November 14, 1919, DS, RG 812.6363/610; Lloyd Burlingham to secretary of state, July 22, 1924, DS, RG 812.6363/1516; and R. D. Hutchinson, "Memorandum relativo a la situación obrera en Macuspana, Tabasco," March 23, 1925, Shell GHC/MEX, D29/1/1.

⁶⁶ "Committee Plan—Petroleum Development Company (Summary)," April 26, 1922, DS, RG 812.6363/1135.

In 1924 another committee of oilmen, led by C. O. Swain of Jersey Standard, came to Mexico hoping to negotiate a reduction in taxes and a compromise on exploration rights. Swain suggested that the matter of ownership of the subsoil resources remain undeclared. The oil companies would obtain private rights to new lands and then seek government drilling permits at *fixed* rates of taxation. But Secretary of Finance Alberto J. Pani insisted on the concept of concessions and on the government's right to raise taxes when it thought fit. In exasperation, Swain suggested that Venezuela was a better area than Mexico in which to develop oil resources. Pani replied wryly that Venezuela was like the Mexico of Porfirio Díaz: Mexico had had its revolution, and Venezuela's would come.⁶⁷ The government and the companies were stalemated and new exploration delayed.

The 1928 Calles-Morrow agreement ended the stalemate. National ownership of the oil deposits was affirmed, but companies that had performed "positive acts" on their Golden Lane properties before 1917 retained their property rights. Although the diplomatic compromise drew criticism from U.S. companies, like Jersey Standard, that had arrived in Mexico after 1917,⁶⁸ exploration began anew. A number of oil firms obtained nearly five million acres of concessions in Nuevo León and Tamaulipas, just south of new oil discoveries in Texas.⁶⁹ (To date, no significant production has been found in this area.) In the mid-1920s El Aguila had secured rights to the Poza Rica properties and successfully opened up the only new oil find in Mexico during this period. Shell geologists had used there the latest innovation in exploration, the seismographical survey of the earth's substrata.⁷⁰ Oilmen at Poza Rica, located at the southern extremity of the Golden Lane, were able to extend existing pipelines and terminals at a modest additional cost. Further exploration in Tabasco, on the other hand, would have required extensive construction of storage tanks, pipelines, and port terminals. This investment seemed dubious in view of the oil glut and low prices of the 1930s, and the companies that had explored Tabasco in 1920 did not return a decade later. The Reforma Fields lay undiscovered for another forty years.

THE HISTORY OF THE MEXICAN AND VENEZUELAN oil industries as revealed in company documents makes clear that foreign oilmen were buffeted by external forces to which they had collectively contributed but which were beyond the control of their individual companies. Supplies, markets, and prices determined and limited their business decisions. Although Doheny and Cowdray developed Mexican oilfields in order to enter the domestic market, overproduction involved them in an international market, where they felt increasingly insecure. Even international marketing giants like Shell and Jersey Standard disliked depending on the big

⁶⁷ "Presentation of Committee," September 22, 1924, DS, RG 812.6363/1529.

⁶⁸ Standard Oil Company (New Jersey) to secretary of state, April 27, 1928, DS, RG 812.6363/2558.

⁶⁹ Jack Logan, "Mexico's Future Petroleum Possibilities," *The Oil Weekly*, December 5, 1930, pp. 30-34; Rippy, *Oil and the Mexican Revolution*, 62; and Meyer, *Mexico and the United States*, 135.

⁷⁰ Assheton to Body, October 27, 1924, Shell GHC/MEX, D29/1/1; and *The Royal Dutch Petroleum Company, 1890-1950: Diamond Jubilee Book* (The Hague, 1950), 34-35, 38.

TABLE 7
United States Imports of Petroleum from Mexico,
Venezuela, and the Dutch West Indies, 1920-35
(in thousands of barrels)

<i>Year</i>	<i>Mexico</i>	<i>Venezuela</i>	<i>Dutch West Indies</i>
1920	106,163		
1921	125,241		
1922	129,142	755	
1923	79,495	810	
1924	73,062	1,186	
1925	55,049	4,691	3,273
1926	40,019	12,205	7,088
1927	26,019	21,332	10,136
1928	17,584	21,987	24,989
1929	12,663	34,431	16,222
1930	10,093	25,299	9,780
1931	8,207	21,122	4,103
1932	7,147	25,645	
1933	5,866	17,218	
1934	4,866	25,304	
1935	3,454	24,597	

NOTE: The oil companies processed Venezuelan crude oil in the refineries of the Dutch West Indies.

SOURCES: A. R. Coleman to H. N. Branch, Tampico, September 22, 1928, National Archives, Washington, D.C., General Records of the Department of State, Record Group 812.6363/2662; and American Petroleum Institute, *Petroleum Facts and Figures* (9th edn., New York, 1950), 339.

independents for supplies of crude petroleum. In order to reduce risk, both producing and marketing firms responded by seeking vertical integration. Prices became the arbiter. In times of rising oil prices and scarce supplies of crude oil, independents who controlled oilfields generally had the advantage, and they expanded into foreign marketing. Cowdray was the exception, because the paucity of British capital during wartime prompted him to sell out to Royal Dutch/Shell in 1919 even as prices were rising. In times of falling prices and abundant supplies of crude oil, the marketing giants expanded by buying out the producers.

The same economic forces worked on the oil-producing nations. Foreign companies were attracted to Mexico by rising oil prices during World War I. In the 1920s, however, falling prices favored the more prolific oilfields. The exhaustion of the wells in the Golden Lane undermined Mexico's competitiveness to the point that even Mexican consumers were welcoming imported oil products. The international companies remaining in Mexico, therefore, were those that either had a share of the growing domestic market or were attempting to profit from the

existing oilfield installations. Real oil interest, meanwhile, had shifted to Venezuela. Here the independents had less success developing the industry because falling prices in the 1920s favored integrated marketing giants like Jersey Standard and Shell. Venezuelan oil increasingly entered the markets being vacated by Mexican petroleum (see Table 7).

And what of politics? In contrast to Venezuela, Mexico was a nation in which the governing elites had been largely excluded from participation in the industry because of the early private property nature of oil production. For eleven years, from the promulgation of the 1917 constitution to the 1928 Calles-Morrow agreement, the government sought to enforce public dominion over a resisting industry. The conflict retarded exploration and drilling programs. By the time that the companies and the government had settled the issue of public dominion sufficiently to permit new exploration in Mexico, cheaper production from Venezuela had captured world markets while prices reached a nadir. It is true that the investment climate provided by Gómez, that friend of industry, benefited foreign oilmen. But Mexico could not match the spectacular drilling and production gain achieved in the Maracaibo region.

The oilmen shifted their interests from Mexico to Venezuela because of the loss of Mexican oil's competitiveness, a loss reinforced but not caused by revolutionary nationalism. The primary power of the foreign companies was economic—that is, their access to capital, technology, and markets on an international scale—rather than political. Although they could not defeat Mexican nationalism, the oil companies did survive it.

Reviews of Books

GENERAL

BARBARA W. TUCHMAN. *The March of Folly: From Troy to Vietnam*. New York: Alfred A. Knopf. 1984. Pp. xiv, 447. \$18.95.

In 30-odd pages of introduction including nine or ten instances plucked from a wide range of history, 16 pages on Troy, 76 on Renaissance popes, 106 on the British loss of North America, 146 on the U.S. in Vietnam, and 8 pages of epilogue, Barbara W. Tuchman has assembled a casebook of rulers' persistence in self-destruction. Like many a casebook, *The March of Folly* has didactic purposes—primarily, to promote the infusion of reason into public affairs; secondarily, to shake contemporary rulers from their rigidities, including the hope “to preserve the present government of the United States from imbecility in El Salvador” (p. 383).

As the allocation of pages suggests, Tuchman's analysis of Vietnam serves as her centerpiece. It is the only segment based on sustained, original reflection and on sources not readily available to the average student. In all the examples she uses, including Vietnam, Tuchman asks how and why groups of powerholders persist in policies that visibly produce effects contrary to their interests and for which alternatives are clearly available. In each instance, she offers a lucid narrative of the powerholders' behavior, lays out her reasons for considering that behavior foolish, and speculates on the reasons for the folly. She makes few factual errors (one of the few is her statement that mutiny was “incomprehensible” to eighteenth-century British troops [p. 145]). She rarely fails to stimulate and entertain. *The March of Folly* is a pleasure to read.

In broaching so broad a subject, Tuchman has taken a dare. She has no choice but to plunge into counterfactual history, speculating on what might have been in the absence of folly. Thus, she almost persuades us that without the personal corruption of Renaissance popes no Reformation would have occurred. Except for an occasional analogy, she eschews comparison, either among her cases or

(more crucially) with similar cases that came out differently. She therefore labors with the terrible burden of hindsight; having selected her instances because the powerholders' decisions turned out disastrously, she must search among all the antecedents of those outcomes for genuine causes of disaster. She finds them consistently—but not always convincingly—in the corruption, ambition, pride, or indolence of particular rulers.

Tuchman tells a good story. She helps us hate the Renaissance popes by feeding us details of their concupiscence, reminds us of the possibility that Lord North was George III's illegitimate son, and sets down quick, acid sketches of Kennedy advisers Robert McNamara and W. W. Rostow. The thirty-nine illustrations, which range from good to superb, give faces to many of her chief characters. Like the illustrations, Tuchman's book deals mainly with notables. Common people figure as chorus or as demiurge—as mere spectators and victims at the siege of Troy, as the Florentine “crowd” and “mob” first stirred and then repelled by Savonarola, and as the English, Irish, and American “mobs” the British government sought to intimidate in the 1760s. Tuchman necessarily discusses American popular reaction to the Vietnam War but suggests repeatedly that campus-based marches and demonstrations had either a minor or a negative effect, if only because inflated rhetoric, destruction of property, long hair, and immoral behavior offended ordinary Americans who, in the absence of disorder, would have seen the folly of Vietnam more quickly. As a former demonstrator, I doubt that. In any case, her real story concerns Roosevelt, Truman, Eisenhower, Kennedy, Johnson, Nixon, and their counselors.

Why all the folly? Tuchman's answer is, regrettably, tautological and unhistorical. It is tautological because “rejection of reason” not only explains folly but defines it as well. Tuchman's feeble suggestions that lust for power and mental standstill of those in power promote rejection of reason do little to break out of her argument's closed circle. Her appeal to the psychologists' notion of cognitive dissonance—that individuals exposed to incompatible cues tend

to suppress enough of the incoming information to make the message coherent—provides no way of guessing which leaders will rigidify their thinking and which will vacillate, compromise, synthesize, or change direction.

Tuchman's treatment of folly is unhistorical because, its title notwithstanding, her book describes no "march," no significant variation or development, of governmental foolishness over time and place. The thirteen or fourteen cases simply provide so many opportunities to recount an interesting tale, cluck one's tongue at the self-destructive folly of great persons, and reflect on the importance of finding leaders who pass the test of character, who display moral courage, and who exercise reason.

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MICHEL FOUCAULT. *Histoire de la sexualité*. Volume 2, *L'Usage des plaisirs*; volume 3, *Le souci de soi*. (Bibliothèque des Histoires.) Paris: Gallimard. 1984. Pp. 285; 284. 170 fr. the set.

These two volumes are quite different in emphasis from the more controversial volume that preceded them, *La volonté de savoir* (1976). That volume dealt primarily with the early modern period and was a further development of Michel Foucault's general theory that the "Enlightenment" had been marked by the "great confinement," the drive to isolate all "deviants" in society. These two volumes are concerned with the classical period, and in them Foucault is trying to explain what he calls the birth and development of Western moral ideas about sex, desire, and pleasure. Perhaps if he had examined this period earlier, he would have been somewhat more cautious in his claims about the uniqueness of the early modern period. Foucault is not interested in sexual practices but rather in concepts, and these are studies more in the history of ideas than in social history. In fact, he emphasized that, although these volumes are offered as a study in history, they are not the "work" of a historian. Foucault is highly selective in what he includes, and he would be willing to agree that, for example, Seneca, Epictetus, and Musonius Rufus did not represent the conduct of the people of their time or even the ideas of a majority; instead, they are part of a tradition that was adopted and incorporated into the body of Christian thought, and it is this body of thought that still dominates much of our thinking about sexuality.

To examine this thought, Foucault turns to the primary sources and does a more thorough mining and exposition of those pertaining to sexuality as he defines it than anyone else. Key to his thinking are

the Aristotelian concepts of *sōphrosunē* (expectation of self-control or moderation) and *enkrateia* (mastery of oneself or one's behavior). It is in these concepts—one external, the other internal—that Foucault organizes his findings. Sexuality is, therefore, conceived as something that has to be controlled in spite of difficulties.

Each chapter begins with a definition, an essential prerequisite since the Greek and Latin terms are not quite the same as the French (or English), and some who have hastily surveyed the field in the past have been led astray. For example, the term *aphrodisia*, the subject of the first chapter, encompasses both more and less than our term sexuality, and it is these nuances in meaning that are important for understanding what the Greek writers were saying. Included in the first volume are discussions on the sexual regime in general, what constitutes pleasure, the risks and dangers associated with sexuality, the place of marriage, what is meant by temperance, and, finally, homosexuality, or, in reality, bisexuality, since it deals with boy love.

The second volume is concerned with later writers (both Greek and Latin), and one section is based heavily on Artemidorus and his work on dreams. Foucault argues that Artemidorus's study is the only surviving complete text of a type of dream literature that was plentiful in the period, and, as such, it is doubly important, because, if it can be taken as typical of the genre, we can look at sexuality in quite different ways than the other extant philosophical and literary texts allow us. This, to my mind, is the most original and, at the same time, the most controversial aspect of Foucault's study. Also included is a section devoted to Galen and medical concepts of sexuality, another on ideas and concepts about women and their sexuality, and, finally, one on changing ideas in the third century about boy love. None of what Foucault says is unknown to individuals who have done work in the field, and, although Foucault mentions his primary sources, he does not seem to have made an effort to look at the secondary literature about the subject in English or German. He is committed to the study of ideas, not social phenomena, and so he does not look at such aspects as Gnosticism, Manichaenism, or even Greek mystery religions to see how much influence they had on ideas. For him ideas beget ideas; although he would probably admit to the importance of external factors, he is essentially an internalist, looking within the scientific and intellectual community for answers. The great importance of Foucault's work lies in his more general reputation as a scholar of our time. He turned his abilities to looking at previously ignored subjects and emphasizing their importance in our understanding of ourselves and past society. He has now done the same for sex, and, in the process, his work ought to convince those historians

who have not yet been able to bring themselves to look at such previously forbidden topics that this research topic is a valuable one and a respectable one to investigate. Unfortunately, his death at 59, shortly after this work was completed, will prevent Foucault from participating in these intellectual explorations.

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FRANCIS OAKLEY. *Omnipotence, Covenant, and Order: An Excursion in the History of Ideas from Abelard to Leibniz*. Ithaca: Cornell University Press. 1984. Pp. 165. \$17.50.

Francis Oakley's purpose in this small but densely packed book is twofold: to vindicate the study of the history of ideas as advocated and exemplified by Arthur O. Lovejoy and to complement Lovejoy's own work on the idea of the "great chain of being" with an examination of a parallel but different idea-complex. As Oakley explains, Lovejoy's method needs vindication because it has been so widely dismissed in favor of other methods and conceptions of historiography. In his first chapter, Oakley offers an extensive (though not detailed) survey of the rival schools of thought and their complaints against Lovejoy. These include the *analists*, Foucault, cliometrics, psychohistory, structuralism, deconstructionism, and so on. Most of these rival schools share a tendency toward various forms of determinism and therefore dismiss the study of the "history of ideas concerned less with contextual factors of a social nature than with the logical analysis of ideas, of their reciprocal interaction or their internal development within an intellectual tradition" (p. 20).

One thrust of Oakley's defense of Lovejoy, therefore, is to show that Lovejoy actually shared many of his critics' concerns about the importance of historical context and was not at all monopolistic in his conception of the value of his method. The other is to demonstrate, by his own exploration of another idea-complex, that the history of ideas still has an important contribution to make. He is highly successful in both efforts.

It is the latter, of course, that is the main subject of the book: the study of the idea that the order of the universe, especially moral and social order, is grounded not in essences but in intentions. Underlying the idea of the great chain of being was the assumption, predominant in the Greek cultural milieu, that the ultimate explanatory principle for everything is to be found in ontological structure. Oakley points out that this assumption was never the only one operative in the history of ideas but that,

since ancient times, it has had a major competitor in the idea, favored in the Hebraic tradition, that order is founded on free decisions of human beings or of God. It is the history of this idea that Oakley makes his own focus in a brief but excellent account of its many variations in European thought from the twelfth to the seventeenth century.

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ROBERT EDEN. *Political Leadership and Nihilism: A Study of Weber and Nietzsche*. Tampa: University Presses of Florida. 1983. Pp. xx, 348. \$25.00.

The title of this exercise in political philosophy fails to mention the central intellectual commitment of the book as a whole: its concern is the supposed present-day malaise of American liberal thought and institutions, which Robert Eden defines as deviations from *The Federalist Papers* and other "classic expositions of modern liberal politics" (p. 186). The introduction disarms the reviewing historian by reaffirming a presentist *engagement*. If Eden had anticipated passage of the Voting Rights Act of 1981 (which, he fears, will further the cause of proportional representation by race), he "would have devoted more space to Weber's vigorous opposition to proportional representation in Germany." Instead, his attention was focused on "what then seemed the most ominous development that a study of Weber might illuminate: the plebiscitary system for selecting presidential candidates in primaries, which governed the campaigns of 1972, 1976, and 1980" (p. xv). Consequently, this political scientist-philosopher does not set out to interpret the works of Weber and Nietzsche in their German setting, but only those parts of their writings concerned with political leadership broadly defined and only as they can be interpreted to bear on the American political system.

This approach leads to twisted hypothetical dialogue. For example, "[Woodrow] Wilson might call attention to Nietzsche's writing on the liberal cause in passages that clearly presaged Weber's argument" (p. 33). It also leads to vague generalizations that the historian finds impossible to prove or disprove. For example, "Weber is superior to Wilson because he adopts the principle that political philosophy is impossible" (p. 217), and "Weber accepted enough of Nietzsche's critique of political philosophy to be convinced that to admit the efficacy of Montaigne, Machiavelli, Bacon or John Locke would be tantamount to abandoning the last defensive against Nietzschean leadership of science" (p. 219).

This mode of scholarly discourse allows the author, in the copious notes as well as in the text, to refer briefly to opinion leaders and political actors

ranging across the span of Western civilization from Socrates to Ronald Reagan and including along the way: Plato, Aristotle, Thucydides, Alcibiades, Caesar, Machiavelli, Bacon, Hobbes, Locke, Burke, John Stuart Mill, Gladstone, Montaigne, Montesquieu, Rousseau, Voltaire, Napoleon, Tocqueville, Raymond Aron, Kant, Hegel, Marx, Bismarck, Heidegger, Hitler, Alexander Hamilton, Martin Van Buren, Lincoln, Wilson, Leo Strauss, Hannah Arendt, and Martin Luther King.

The first chapter contrasts the American founders' concept of limited government and "Madisonian interest group representation" with the "issue-oriented opinion leadership" Wilson strove to institute (p. 31), which in turn is compared to Weber's (inapt) defense of and Nietzsche's rejection of liberal institutions. Among several other points, the second chapter seeks to demonstrate that Nietzsche's history of the origins of the liberal cause "is inseparable from his explanation of why philosophy must at present, and for the future, turn against liberal institutions" (p. 59). In the third chapter Eden ponders Nietzsche's and Weber's explicit and implicit attitudes toward "the disharmony and contradiction between the method of democracy and the method of science" (p. 72). The fourth chapter argues that Weber's justification of leadership is built on the recognition of the dangers inherent in the "new individualism," which Eden considers a fundamental threat to the American type of constitutional democracy (p. 99).

In the fifth and sixth chapters the author weaves a multitude of inferences around Weber's well-known defense of *Sachlichkeit* required by science against the vogue of Nietzschean irrationalism and around Weber's famous distinction between an ethic of responsibility or consequences (*Verantwortungsethik*) and an ethic of conscience or principle (*Gesinnungsethik*), which is related to Nietzsche's "master morality." Weber's approval of "plebiscitary leader democracy" is criticized for legitimizing demagoguery, renouncing principles of liberal representation, and attempting "to detach liberal democracy from a standard of human rights grounded in nature" (pp. 188–89). In the final chapter Eden deplores the "precritical" (p. 222) use of the concept of political leadership in contemporary American political science and sociology and presents his own political philosophy: righteous conservatives as well as reform liberals should now admit the necessity of protecting constitutional democracy as outlined in *The Federalist Papers* against "self-selecting opinion leaders" (p. 237), "potential tyrannical majorities" (p. 225), the blurring of the boundaries between the public and private spheres, and other dangerous elements of plebiscitary leader democracy.

Because the philosopher's explicit intention is to reach the (busy) American opinion-leading and

-governing elites with his call to return to the foundations of American liberalism, it is difficult to understand why Eden did not formulate and organize his arguments more comprehensibly.

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STEVEN B. SMITH. *Reading Althusser: An Essay on Structural Marxism*. Ithaca: Cornell University Press. 1984. Pp. 230. \$19.95.

The reputation of Louis Althusser has grown steadily in the Anglo-American world since the translation, in 1967, of the essays originally published under the title *Pour Marx*. Both the details of Althusser's idiosyncratic "reading" of Marx and his larger theoretical project of a "structural Marxism" have, in the ensuing decade, triggered a dense and generally acrimonious controversy. At stake are the status of Marxism as a theory and the proper domain of its study: the historical evolution of forms of domination, or the reciprocal interaction of given forces within a structured totality. To partisans, Althusserian Marxism promises a means of grounding the theory firmly and finally on secure epistemological foundations; to critics, it represents a disturbing theoretical aberration that sacrifices on the illusory altar of rigorous social science precisely what is original and significant in Marx's thought.

Steven B. Smith's *Reading Althusser*, though highly critical, is more than another contribution to the ongoing polemic. To the contrary, even those who are favorably disposed toward Althusser's theoretical undertaking will have to appreciate the fair presentation of his principal conceptions and to respect Smith's independent and carefully argued objections to them. With the concept of "theoretical practice," Althusserians seek to assert rather than heal the breach between the theoretical and the empirical, and all sides, I think, will be grateful for the exceptional clarity with which the arcane formulations that often result are elucidated in this stimulating "essay on structural Marxism."

A further virtue of this book lies in the author's determination to elaborate what Althusser himself calls the *conjoncture* of historical and theoretical forces within which his theoretical position is situated. Smith provides a chapter on the currents of "Western," or "humanistic," Marxism against which Althusser offers his scientific corrective, and, although readers familiar with Lukács, Kojève, and Sartre will find nothing original here, the philosophical work of these authors is linked together in such a way as to establish a necessary theoretical context.

Although explicitly remaining within the domain of political theory, *Reading Althusser* nonetheless performs the salutary task of indicating to historians

why they should consider Althusser's arguments. Precisely because the Althusserian project is to purify theory of all empirical contamination and specifically "to establish Marxism as a scientifically grounded social theory with no debt to history or to the methods of historical research" (p. 71), traditionally minded historians may be inclined to dismiss it as an affront. They would do better, however, to recognize that it embodies a theory of enormous consequence, one that intersects, as the author demonstrates, with some of the most powerful and influential currents of contemporary psychology, sociology, and linguistics.

In the last analysis, Althusser's structural Marxism completes the displacement of the individual subject from the center of the historical stage, an effort that has been underway almost from the time that the liberal theory of possessive individualism was announced in the seventeenth century. In its place, one is left to contemplate concrete, objectively knowable structures of social and economic life, "an eternal combination of a finite body of elements that provide the a priori building blocks of any possible mode of production" (p. 180).

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NOËL O'SULLIVAN. *Fascism*. (Modern Ideologies.) London: J. M. Dent and Sons; distributed by Biblio Distribution Center, Totowa, N.J. 1983. Pp. 223. Cloth \$19.95, paper \$7.95.

This lively little book starts from a breezy premise: scholars have not explained fascism; they have merely explained it away. Noël O'Sullivan argues that the reason is this: by all that is holy in our good old liberal optimism, fascism should not have happened. Scholars have thus interpreted it as "a bolt from the blue," an aberration in European development, a breakdown, a conspiracy, anything but what O'Sullivan submits it to be—a movement in the direct line of the Western political tradition.

The big questions about fascism, O'Sullivan insists, will not be answered by "culling new facts from the archives." Required, instead, is a new and different interpretative concept. In brief, here is one more attempt to define a concept by inventing another. In this case it is called "the activist style in politics," described as a tradition of thought developed in opposition to an older "limited style of politics." The limited style (read liberalism) advanced the ideals of law, individualism, and the constitutional state; the activist style placed ideology above law, the mass above the individual, the movement above the state. From Rousseau to Hitler, from Napoleon to Lenin, thinkers in this tradition turned liberal ideas inside out, representing despot-

ism as the "true liberty" and converting politics into permanent revolution. Such is O'Sullivan's vision of a radicalism that, he believes, has driven all Western revolutionaries, Jacobins, communists, fascists, the Red Brigades, terrorists of every sort. To this tradition, fascism added nothing new; its ideology was elementary: the leader is always right. Fascism, simply, was the activist style in heat. O'Sullivan concludes, "The sole distinction of fascist ideology lies in the fact that it may be regarded as the most extreme, ruthless, and comprehensive expression of this new style of politics" (p. 41).

All fascist parties, therefore, were alike in their generic connection to this tradition. For O'Sullivan, however, real fascist parties amounted to just two: Italian Fascism and German Nazism. Fascisms elsewhere, he vouches, were mere "copies" of these two movements. Too simple? Never mind; his news about the activist style creates enough confusion on its own. By ordering the most contrary ideas and influences into a single revolutionary tradition, O'Sullivan makes a muddle of otherwise familiar material. For contrary to impressions here, a vast number of works interpret fascism not as "a bolt from the blue" but as the result of longer historical traditions. Indeed, the influence of this scholarship can be found throughout O'Sullivan's pages—and, frankly, not always with sufficient acknowledgment. Present, for example, is J. L. Talmon's concept of totalitarian democracy, Fritz Stern's theme on the politics of cultural despair, and George Mosse's notions on "the new politics." O'Sullivan has style, and his achievement is to work these ideas together in a way that undergraduate readers will find brisk and clear. In the end, however, they are likely to be more confused about the activist style than they are about fascism.

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RICHARD A. VAN ORMAN. *The Explorers: Nineteenth-Century Expeditions in Africa and the American West*. Albuquerque: University of New Mexico Press. 1984. Pp. xiii, 243. \$19.95.

In the vast literature of the history of exploration and discovery, relatively few pieces represent significant departures from what has come to be viewed as the norm. Richard A. Van Orman's book attempts a comparative evaluation of the process of exploration in widely different geographical areas and, given this comparative context, is somewhat of a deviation from the more traditional fare. Like many deviations, Van Orman's book is only a marginally successful, albeit valuable, addition to the literature. The author examines the process of exploration from a broad and cross-national perspec-

tive, and this is a good and worthwhile approach. But his definitions of "broad" and "cross-national" prevent the work from achieving a full measure of success. In this purportedly comparative work, Van Orman investigates only African and western American exploration in the nineteenth century and focuses only on British and American explorers. There are valid reasons for such a comparison: the motives of British explorers in Africa and American explorers in the West were similar in nationalistic, scientific, and moral terms; the results of both sets of exploration rationalized the imperialistic and expansionist goals of the participating national groups. Yet in spite of these obvious similarities there are so many differences between the two sets of exploratory processes that Van Orman's attempt to draw parallels often seems contrived and artificial.

This limitation notwithstanding, *The Explorers* is a thoughtful and thought-provoking piece of work. The careers and expeditions of sixty explorers (thirty from each geographical region) provide Van Orman's point of departure, and he analyzes, in greater or lesser detail depending on the relative importance of the explorer and expedition, the mosaic of individual enterprise, personal bravery, scientific interest, and national ambition that marks nineteenth-century exploration. Both the lands under exploration and their native inhabitants are discussed, as are the patrons and sponsors of the most important expeditions. Van Orman attempts to get at the collective personality of the explorers themselves and to study the nature of the exploratory process from tactical and logistic points of view. He also examines the many studies of exploration, along with the impact of explorers on their time and place, and includes an excellent chapter on the role of the explorers within the framework of Enlightenment scientific traditions. Although Van Orman refers throughout the volume to the works of historians and geographers, he makes little mention of the rich store of ethnographic and ethnological material from African and American exploration. This is a serious gap—and one the author recognizes. The notes are all placed at the end of the book, an all-too-common practice that is as disturbing in a book that is a potentially valuable research tool as is the absence of a bibliography.

The basic idea behind *The Explorers* is a good one, and one wishes that there were more books of this genre and approach. In the last few decades much significant systematic work has been done on exploration, and some synthesis is, at this juncture, necessary. For all its promise, however, *The Explorers* falls somewhat short of the ideal. A book that promises to be outstanding turns out to be merely good, interesting rather than enthralling. It points the way for future efforts rather than providing a

seminal model of the comparative approach to the history of exploration.

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WILLIAM H. BLANCHARD. *Revolutionary Morality: A Psychosexual Analysis of Twelve Revolutionists*. Santa Barbara, Calif.: ABC-Clio. 1984. Pp. xxxiii, 281. \$42.50.

William H. Blanchard's book is neither a narrative parade of modern revolutionaries nor a historical synthesis attempting to integrate theories and data on revolutionary leaders. Its purpose is analytical: to identify the role of a particular personality dimension in revolutionary behavior by examining the life histories of certain notable rebels. This aim coincides to some extent with E. Victor Wolfenstein's *The Revolutionary Personality* (1971) and Bruce Mazlish's *The Revolutionary Ascetic* (1976). But, although the subject matter of Blanchard's book sometimes overlaps that of earlier studies, its perspective and line of inquiry prevent it from being redundant.

Blanchard's point of departure is to pose the question, what part did sexual masochism—"the sensuality and the virtuous thrill derived from suffering" (p. x)—play in motivating and shaping the activity of modern revolutionists? By Blanchard's definition the revolutionist is one who sides with those who suffer and are downtrodden. This excludes, for example, Hitler who, though for long a "rebel" and by reliable accounts a sexual masochist, "never made a secret of his belief in elitism" (p. xxv).

Blanchard's twelve subjects are not presented chronologically. T. E. Lawrence precedes Rousseau, Tolstoi, and Peter Kropotkin. Gandhi comes before Marx, whose case is followed by Sun Yat-sen, Trotsky, and Ché Guevara. Fidel Castro, Lenin, and Mao Ze-dong conclude the list. This grouping is based on a performance criterion: in progressing from Lawrence and Rousseau to Lenin and Mao, Blanchard follows a transformation from one extreme to another in attitudes toward power. Both Rousseau and Lawrence became disgusted with the ambitions that had brought them political fame; both recoiled—the first with guilt; the second, shame—from worldly rewards and acclaim. By contrast, the last three figures—Castro, Lenin, and Mao—presided with apparent confidence over the revolutionary administrations they had helped to install.

The hypothesis that Blanchard offers seems to be: the nearer the sexually masochistic motive is to the surface, the less purposive rebellious activity is. Thus, extreme sexual masochism foretells inhibition of political aims. In contrast, "moral masochism"—sexual masochism transformed into a linking of the

self to the oppressed—may result in intense political activity but produce no sustained impulse to dominate. Only with freedom from masochistic motives (through, for example, sublimation) can energy be used consistently in a revolutionary leadership role.

In Lawrence's writings, as in Rousseau's, consciousness of sensual pleasure at the thought or fact of being physically damaged is evident. At the opposite end of the continuum, Lenin usually transformed whatever tendencies he had to damage himself. Blanchard does not perceive Lenin's "ascetic and self-denying behavior"—or that of Mao and Castro—as "demonstratively masochistic" (p. 232). He does not mention the fact that Lenin, like many world leaders, occasionally suffered from depression, a nonpleasurable form of self-punishment.

Blanchard is least strong on the etiologies of the diverse dispositions of his protagonists. His observations on their childhoods seem based on a more restricted use of sources than do his inquiries into adult performances. Blanchard does give insight, however, into one significant but largely neglected consequence of certain child-rearing practices of modern European elites. He shows that, as children, the meaningful family for Tolstoi, Kropotkin, and Trotsky consisted of paid or indentured servants. Each child, though nominally master of those who nurtured him, was actually, like them, a slave and felt himself one with those who defended him against the dangerous ruling powers—in these cases, particularly, the father.

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RICHARD D. MANDELL. *Sport: A Cultural History*. New York: Columbia University Press. 1984. Pp. xx, 340. \$24.95.

English-speaking sports historians do not always acknowledge the unflattering fact that European, especially German, scholars have long dominated the field. Richard D. Mandell is an exception. In his preface he pays generous tribute to Germanic scholarship and to his German colleagues. He is not, however, simply a borrower. His earlier books on the modern Olympics have stimulated German as well as American research; from his seminal essay on the invention of the sport record grew a fruitful international controversy. The great virtue of his cultural history of sport is the sophisticated theoretical structure that provides form for the ambitious narrative. No critic can say that this book is a collection of sports trivia.

Understanding that definitions are crucial, Mandell defines sport as "competitive activity of the

whole human body according to sets of rules for purposes ostensibly or symbolically set apart from the serious, essential aspects of life" (p. xvii). This somewhat arbitrary definition clearly excludes activities often considered to be sports, like going to the beach or playing Monopoly. By including purpose within the definition, Mandell calls attention to his theme: whether a given activity is sport depends not on biomechanics but on culture. Hence, the *sport* of archery was not practiced on the field of Agincourt.

Armed, then, with a theory about the nature of sport, Mandell examines the ancient world to trace the emergence of sport from its origins in material necessity and cult. He describes sport's first zenith in classical Greece and its spread throughout the Hellenistic and the Roman world. Especially important for Mandell's argument about the constantly changing cultural significance of sport is his chapter on Chinese, Japanese, Islamic, and pre-Columbian sport. After commenting on medieval sport, he turns to the emergence of distinctively modern sport, characterized by a high degree of quantification and an obsession with records, that is, with the ultimate quantified achievement. Quite correctly, Mandell concentrates on England and the United States (for the development of such sports as soccer and track and field) and on Germany (for the systematic development of physical education and gymnastics). In these chapters and in those on the modern Olympics, much of the information is familiar, but old facts about Babe Ruth or the Henley Regatta are seen in a new focus.

Similarly, the final chapters of the book rejuvenate some extremely tired questions about such matters as amateurism, the excesses of intense competition, and the aesthetic dimension of sport. That sport is a social institution shaped by the same historical processes as other modern institutions may be a truism, but it is nonetheless true. An ounce of historical context is worth a pound of moralistic dither. In these final chapters, Mandell admits that modern sport can destroy athletes driven—by commercialism, by nationalism, by their own culturally induced obsessive needs—to physically and psychologically ruinous levels of achievement. Granting Marxist critics of sport their due (and no more than that), Mandell concludes with a defense of "joyous sport, beautiful sport" and with an inspired commentary on sport in the arts and as an art. Finally, there is a splendid bibliographical essay.

How can Mandell be faulted? Inevitably, a one-volume history that ranges from Amenophis II to *Chariots of Fire* will be selective. Incompleteness is one of the rules of that historical game. More troublesome is the forcefulness of many of the judgments. For instance, "From the very beginning, the overwhelming mass of [American] sports journalism has been slovenly, childish, and venal" (p.

185). Perhaps the moralist in Mandell *should* occasionally pin the historian to the mat, but I would prefer a somewhat more balanced match. After all, this superlative book is now—to change the sports metaphor—the mark to shoot at.

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ANCIENT

W. ROBERT CONNOR. *Thucydides*. Princeton: Princeton University Press. 1984. Pp. xii, 266. \$30.00.

In stimulating and well-written chapters devoted to each of Thucydides' eight books, W. Robert Connor directs his attention to the likely reactions of the "sympathetic" reader of Thucydides, thus substituting for a conventional study of "the work and its author" one centered on "the readers and the work." From this perspective, "the issue is not Thucydides' views, but ours" (p. 237); what matters is "not the history of Thucydides' own thinking about the war, but the way in which the reader's understanding is directed and deepened" (p. 143). The hypothetical reader, moreover, is injected with prior knowledge of the war so that he can juxtapose events of the present, past, and future, and he seems to be as interested in the associations of a speech as he is indifferent to its historical credentials.

Thucydides is similarly supposed to have written his history for precisely this readership (for example, pp. 12, 15, 93), a consideration that encouraged him to employ his factual record as the vehicle for a tacit dialogue with his cognizant readers about the war itself and the issues it raised (pp. 26, 34, 42, 59, 72, 216). Connor consequently assumed that Thucydides' apparent objectivity is in reality a "literary strategy" devised to coax the reader into active judgmental involvement with the text (pp. 6–9, 17, 232).

Whether the reactions of any reader tell us more about the object of study than they do about the reader is left open to question unless it is not also demonstrated that the reactions have been deliberately induced by the writer. But other difficulties arise. The strategy of feigned objectivity imputed to Thucydides seems anachronistic as well as ill-suited to one who scorned the rhetorically enhanced "prize composition of the moment" (1.22.4). Troublesome also is the identification of Thucydides' intended audience with readers already familiar with the events and personalities of the war. The assumption is perhaps mandatory if the dialogue sustained by Thucydides and his reader is to invite comparisons with events forward in time. This view, however, entails an adventurous theory of composition (revi-

sion after 404 B.C.). It is not credible that the material collected by Thucydides with such difficulty and fused together into the selective pattern dictated by his powerful intelligence already floated in the public mind. Nor does the alleged technique suit the implications of his role as a strictly contemporaneous historian (1.1) ascertaining the exact nature of events for future generations (compare 1.22.4 with 5.26.1) because of the intrinsic importance of the record (for example, 1.23.5). This is to deny, of course, not that Thucydides could be allusive but simply that any such preconceived principle would have been desiderated by him. Here we must firmly dissociate his methods from Herodotus's, whose distance from the great war of Xerxes facilitated the exploitation of precisely this technique.

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JANE HORNBLOWER. *Hieronymus of Cardia*. (Oxford Classical and Philosophical Monographs.) New York: Oxford University Press. 1981. Pp. xi, 301. \$44.00.

KOSTAS BURASELIS. *Das hellenistische Makedonien und die Ägäis: Forschungen zur Politik des Kassandros und der drei ersten Antigoniden (Antigonos Monophthalmos, Demetrios Poliorketes und Antigonos Gonatas) im Ägäischen Meer und in Westkleinasien*. (Münchener Beiträge zur Papyrusforschung und Antiken Rechtsgeschichte, number 73.) Munich: C. H. Beck. 1982. Pp. xi, 207.

Jane Hornblower has pieced together a credible representation of a valuable lost narrative describing struggles among the "successors" of Alexander. The author of this history, Hieronymus of Cardia, served as an official under four different kings and was eyewitness to many of the events he described in his account. Unfortunately, his book did not survive the Roman Empire. Hornblower has now managed to recover the outline and many details of this lost history by analyzing identifiable fragments and by extrapolating the rest from the works of later historians who appropriated portions of Hieronymus's account.

In particular, she dissects Diodorus and Plutarch, Greek writers in the early Roman Empire. Diodorus's "universal history" epitomized many earlier writers; for the early Hellenistic period, Hornblower's X-ray literary criticism shows that he "swallowed whole slices" (p. 17) of Hieronymus into his compendium, even borrowing Greek terms not customary in other parts of his writings. Similarly, in Plutarch's *Lives of Eumenes, Demetrius, and Pyrrhus*, she detects sections of Hieronymus's work and even viewpoints that Plutarch adopted.

Her chapter on the "truthfulness" of Hieronymus (pp. 107–79) is an attempt to compare data taken from Hieronymus's works with Babylonian cuneiform records, epigraphic and numismatic evidence, and archaeological finds. She identifies seventy-four citations from treaties, decrees, and letters (found in later authors) that were likely first quoted by Hieronymus, who had access to such documents. But the tendency of later borrowers to add sensational details and the absence of any continuous alternative record for the period leaves the accuracy of his lost history unproved.

Hornblower's reconstruction of Hieronymus is based partly on a review of fourth-century historians, yet even more on speculation about how Hieronymus entitled, organized, and concluded his history. She recognized that "Tarn's instinct" in one such conjecture had been "sound" (p. 90) but that "arguments from probability" were not conclusive (pp. 91–92). Her erudite guesswork nevertheless builds conscientiously on classical scholarship of the last century.

In chapter 5 Hornblower ventures into especially intricate deduction to assay the historical and political values of Hieronymus by extracting them from the vocabulary used by historians of a much later era. This hypothesizing seems the least satisfactory part of her intriguing book. From biographies by Plutarch and Nepos she can certainly identify incidents and evaluations of character that must have been preserved by a contemporary of the Hellenistic generals, but her depiction of Hieronymus's ideals is more thought-provoking than convincing.

In a more traditional monograph Kostas Buraselis focuses on events and policies from 315 to 239 B.C. He has summarized a century of Hellenistic scholarship by fitting into sequence the work of more than seventy scholars, correlating data from fifty-eight ancient writers and from papyri, coins, some archaeological evidence, and over two hundred inscriptions.

Buraselis's goal was to clarify the policy of each king by examining the periods of war and peace with other kingdoms and cities. No previous publication seems to have researched in detail such a lengthy period for this pivotal segment of the Aegean. The author does not propose major fresh interpretation, but, by neatly synthesizing a diversity of tattered evidence and opinion, he has made a significant step in Hellenistic political history. He has disregarded economic, social, and cultural aspects of the period.

After an introductory review of the fourth-century rule of Cassander (315–298 B.C.), he traces the development of Antigonid policy by Antigonus I (Monophthalmos) and Demetrius in their struggles against a coalition of enemies. Antigonid sea power and the evidences for an Island League (Nesi-

otbund) before 286 B.C. occupy most of the second section of Buraselis's book.

The most controversial issues during the long reign of Antigonus Gonatus (281–239 B.C.) were wars involving Antiochus I of Syria and a series of battles with Egypt. Chronology rests on limited evidence tangled by difficulties in dating. Although the puzzle is not yet clearly resolved, Buraselis has arranged a plausible "historische Rekonstruktion" (pp. 151–76).

Both of these publications have fine indexes and appendixes giving well-arranged data, which should help further research. Hornblower's appendix supplying the Greek texts of Jacoby's fragments with her commentaries and also her review of late Hellenistic Greek terminology in Diodorus should interest classical scholars. Buraselis provides a chronological list of thirty-eight inscriptions relating to the Island League, summarizing the content and literature on each and updating earlier lists, and he includes an index of all ancient sources, inscriptions, papyri, and coins, correlating these with footnotes in his text, a helpful feature both he and Hornblower add to their more general indexes of names and sites.

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PATRICIA COX. *Biography in Late Antiquity: A Quest for the Holy Man*. (Transformation of the Classical Heritage, number 5.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xvi, 166. \$25.00.

In *The Making of Late Antiquity* (1978) Peter Brown pointed out that a major change in that period was the shift in the technique by which man sought contact with the divine. The locus of the divine on earth became centered on the exceptional man rather than on institutions (p. 13), which makes the importance of biography as a form of religious discourse for both Christian and pagans in late antiquity understandable. Patricia Cox attempts to trace the interplay of biographical form and the concept of the holy man, which provides the foundation for the use of biography as a vehicle for creative mythologizing and for developing an ideal in which history was distorted, if not actually lost (p. 145).

This work falls into two parts. The first is an examination of the development of biography as a genre and is heavily dependent on A. Momigliano's excellent work, *The Development of Greek Biography* (1971). Crucial to Cox's argument is a radical separation of history and biography as genres, each using different standards of truth. Cox asserts that sustained historical veracity is not characteristic of

ancient biography and that this is particularly true of philosophical portraits (p. 5). Although not strictly writing biography, Plato and Xenophon presented a mixture of fantasy and reality to suggest a supramundane truth, which became a feature of this genre and allowed it to become a means of philosophical evaluation and polemic. This line of development culminated in Aristoxenos of Tarentum, whom Cox credits with first using full-fledged biography in this manner. But the evidence on earlier writers such as Dicaearchus is too scanty for such a positive assertion. By the first century A.D., biography had become an established genre ready to serve as a vehicle for philosophical propaganda.

Cox's version of biographical development is too narrow. It ignores the relationship of philosophical biography to the novel (a problem dominating the classical case of the biography of the holy man, Flavius Philostratus's *Life of Apollonius of Tyana*), while failing to account for the concern of biographical writers to present a true portrait of their subjects. Philostratus believed it necessary to invent the memoirs of a student and companion of Apollonius. Late pagan biography frequently stressed the truth of its characterization (for example, Eunapius's VS 1.1.5).

In handling the concept of the holy man, Cox is on firmer ground. She correctly states that, in contrast to the miracle worker, the holy man is characterized by a constellation of traits, focusing on the possession of supernatural wisdom and concern for mankind. It is his mission to communicate this wisdom, expressed outwardly by an ascetic form of life. She distinguishes two types of holy men, "god-like" and "son of God." The latter's most obvious trait is working miracles. This appears overly rigid but is useful.

The second, and less successful, section of the book is an application of the typology developed in the first, using the portrait of Origen in the sixth book of Eusebius's *Ecclesiastical History* and Porphyry's *Life of Plotinus*. The reconstruction of Origen's life, which Cox uses to control Eusebius's narrative, is based on a hypercritical and, at times, inconsistent reading of the sources. The discussion of Porphyry fails to take into account the differing degrees of reliability accorded to statements based on hearsay and of Porphyry's actual experience. It also ignores the context of the life, which is an introduction to and a polemic for Porphyry's edition of Plotinus's treatises.

Although Cox's book is flawed in conception and at times turgid in style, it has valid points to make. It is a welcome sign that interest is growing in the study of late antique biography.

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RICHARD J. A. TALBERT. *The Senate of Imperial Rome*. Princeton: Princeton University Press. 1984. Pp. xvii, 583. \$60.00.

This is the first comprehensive survey of the Roman Senate during the imperial period. In order to discuss this topic for the years 30 B.C.–A.D. 238, Richard J. A. Talbert had to collect and peruse a vast amount of evidence. The literary, epigraphical, legal, and papyrological sources mentioned in the book are valuable as a corpus and are listed in an index. Numismatic evidence was also used; evidence from sculptures is mentioned but not analyzed in detail. Even more impressive than this harvest of relevant evidence is the author's reliance on ancient sources as opposed to received opinion. His knowledge of modern scholarship is exhaustive, and the major works are in the bibliography. But Talbert is unwilling to push his conclusions further than the evidence from ancient sources strictly warrants.

In the first part of the book, "The Corporate Body," Talbert outlines where the senators came from and what their legal and social status was. Many were not wealthy enough to support their financial obligations without strain (p. 53). The second part, "Sessions," describes where and when the Senate met as well as its procedures. The author explains the emperor's role during meetings. He believes that the Senate met for substantially more than the minimum twenty-four sessions a year, partly because of its function as a court of law (p. 215).

The powers and responsibilities of the Senate are discussed in part 3, "Functions." Because Talbert does not explain the constitutional basis of the relationship between the Senate and the emperor, one has to consult Mason Hammond's *Antonine Monarchy* (1959). Talbert uses it and generally agrees with its conclusions that, for example, the Senate conferred power on the emperor but could abrogate it as well (Talbert, pp. 354–56; Hammond, p. 257) and the Senate had the authority to deify a deceased emperor or to condemn his memory (Talbert, pp. 387, 357; Hammond, p. 258). Talbert has unfortunately omitted mention of T. Mommsen's outdated but seminal concept of dyarchy, but he makes a good point in part 2 when he says that, although the emperor was supreme, no emperor could seriously alienate the senatorial class and survive (p. 164). He describes the emperor's control of the army on pages 425–27.

The author concurs with the modern view that the spheres of administration of the emperor and the Senate were not rigidly separated. Besides other areas, this applies to trials, the provinces, the treasures, and coinage. He explains the use or omission of S(enatus) C(onsulto) on coins as reflecting "the whim of, say, the official who chose the design or its

engraver" (p. 382). In the opinion of this reviewer, it would not be going too far to agree with D. W. MacDowall that the senatorial reference was one of courtesy alone (*The Western Coinages of Nero* [1979], p. 70). Of particular value in this part is Talbert's list of senatorial legislation, namely, 234 *senatus consulta* and imperial speeches to the Senate.

Eighteen pages of appendixes and eight pages each of additional notes and a glossary follow the third part. Appendix 9 contains a list of thirty-six senatorial trials of provincial governors for misgovernment. The book contains eight plates, showing the Curia Julia, where the Senate usually met, and coins. The proofreading is excellent, but read "latus clavus" on page 574 in the general index. The accuracy and organization of this comprehensive work are admirable. This is not only the definitive book on the subject but is also a pleasure to read because of the clarity of Talbert's writing and the validity of his conclusions.

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RALPH MERRIFIELD. *London: City of the Romans*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xi, 288. \$30.00.

Throughout the past century and into our own, explorers, looters, and archaeologists set forth from London to uncover the more spectacular remains of antiquity all over the world while neglecting London's own Roman antiquities, which were being extensively destroyed. As the foundation diggings of Victorian and post-Victorian office buildings displaced shallower structures in the City of London while water lines, sewers, and railway tunnels went yet deeper, there was no systematic rescue archaeology to preserve the remaining record of Roman Londinium located within the city's boundaries. It is only recently, basically since the Second World War and especially in the last two decades, that serious archaeology has been accomplished on a large scale. Much remains to be done, in conditions exceedingly difficult, for the site clearing of the Blitz was followed in the postwar years by yet more intrusive construction, which awaited no archaeological dispensation.

Although from the first the chief place of Roman Britain, and "teeming with merchants" in the time of Tacitus, Londinium was never a major metropolitan center; because the entire province was insecure, it was never much more than a glorified frontier town, marked by rude utilitarian constructions rather than the well-statued fora, ample baths, and elaborate circus-theaters of the more fortunate cities across the water. Still, it did have some mosaic-floored villas and the inevitable political monu-

ments and temples if only on a modest scale, in addition to the necessary fort, wall-curtain, river-side godowns, wharfs, and, above all, the bridge (of which no remains have been recovered as yet). It was that Roman bridge across the Thames that brought Londinium into existence and assured its resilient prosperity.

With their genius for places—and genius was needed with mapping grossly inaccurate—the Romans identified the area just above the tidal limits of the Thames as the most profitable urban center for their new province, especially because the Thames estuary is almost opposite the estuary of the Rhine and can thus be connected with the major transport artery of Western Europe. Once the bridge was built, Londinium grew at the intersection of three major traffic flows: the river gave access to the Thames Valley and thus to the core of southern England; the network of Roman roads converged on the bridge; and the port served shipping to and from the Continent. That London was a Roman creation is now virtually certain. No trace of a pre-Roman agglomeration, or *oppidum*, has been found, which is scarcely surprising, for, without the Roman-built bridge, *oppida* would more naturally emerge around fords further up the river. Geoffrey of Monmouth's twelfth-century conceit of a Trojan founder needs no refutation, while his further offering of a pre-Roman *civitas Trinovantum* reflects a late-Roman writer's confusion of the earlier meaning of *civitas* (tribe) with "city." Likewise Francis J. Haverfield's exquisitely Victorian theory of a city spontaneously created by merchants and artisans, a Roman Birmingham, no longer persuades: it was the state-built roads and state-built bridge that made Londinium; the merchants followed.

Even if lacking in the spectacular finds that excited old-style practitioners, the archaeology of Londinium is of surpassing interest both in itself and because it registers the vicissitudes of Roman Britain, faithfully reflected precisely because London remained a frontier town. With today's scientific archaeology, the evidence extracted from even the most modest finds of routine constructions, humble pottery, coin hoards, and scarce inscriptions can be revealingly collated to tell us more of the history of London and Britannia as a whole, on which the narrative sources are notoriously poor. And, if no galleries can be filled with statues (though there are some), the careful dissection of small finds brings to life the vignettes of everyday life, crime included (as in the forger's coin hoard illustrated in the book).

Having started so belatedly, London's archaeology is now so energetic that Ralph Merrifield's previous work, *The Roman City of London* (1965), is already obsolete. His new, well-written book once again provides a judicious overview and synthesis of the accumulated evidence, set in the context of a

brisk but fairly comprehensive narrative. The maps and illustrations are sufficient in number, well reproduced, and very much to the point. But, attractive though it is, this is no mere picture book. As the archaeological work proceeds, it will eventually lead to publication of detailed monographs and specialized articles. This book provides an excellent interim account of respectable scholarship. It should attract a wide readership.

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MEDIEVAL

PETER CLASSEN. *Studium und Gesellschaft im Mittelalter*. Edited by JOHANNES FRIED. (Schriften der Monumenta Germaniae Historica, number 29.) Stuttgart: Anton Hiersemann. 1983. Pp. xx, 305. DM 98.

Among the *Nachlass* of Peter Classen, who met an untimely death in December 1980, were seven papers devoted to the theme of education and society in the Middle Ages. These are collected in this splendid volume edited by his student Johannes Fried. Eschewing the traditional history of educational institutions, Classen investigates the interaction between schools, scholars, politics, and society in a manner not unlike Alexander Murray's in *Reason and Society in the Middle Ages* (1978).

The introductory chapter, which republishes a paper that became a classic in 1966, declares and illustrates the underlying theme in the French schools of the early twelfth century. The last two papers, which have also been published previously, meditate on the meaning of the medieval university for modern times. The longer of the two explicates *libertas scholastica* by distinguishing the modern connotation of freedom of instruction from the medieval conception of liberty as the sum of scholars' rights and privileges, including their right to subject all questions to the scrutiny of scholastic disputation. Classen concludes that medieval experience can pose questions for modern times without presuming to furnish the answers. Another paper, following in the footsteps of Pearl Kibre's *Scholarly Privileges in the Middle Ages* (1962), analyzes reform within medieval universities by comparing the development of their structures during the first third of the thirteenth century.

The central and most original part of the collection, however, consists of three chiefly unpublished papers that call on prosopography to uncover the social context in which schools and scholars operated. In the first, devoted to Italy, Classen compiles

lists of municipal officers and judges and identifies those who displayed legal training. When this evidence is compared with the appearance of local law codes, he is able to ascertain the emergence of legal studies at Milan and Pisa in the early twelfth century, exclusive of the influence of Bologna. Pisa, in fact, has the distinction of being the first medieval polity outside of the church to require legal training for entrance to municipal office. The second paper explores the connections between the schools of Paris and the Roman curia by detailing the careers of popes and cardinals who were known to have studied at Paris in the twelfth and thirteenth centuries. Notoriety and success at Rome thereby serves to cast light on a small but significant sample of Parisian alumni. In a third paper the development of royal courts and the practice of common law in England are reviewed. By identifying the royal judges, both clerical and lay, of the thirteenth century and by establishing their family, scribal, and teacher-pupil relationships, Classen is able to fashion a social context that helps explain the unique character of Henry of Bracton's *Note Books* and *Treatise* and distinguishes the English system of legal apprenticeship from the more formal training of Continental schools and courts.

Much of the ground covered in these studies will be familiar to students of medieval education, but readers will nonetheless find Classen's widely ranging explorations over Italy, France, England, and Germany to be remarkably careful, judicious, and abreast of the latest scholarship in European languages. Most important, they will see the achievements of medieval schools through an optic focusing on the society that nourished these schools.

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TIMOTHY GREGORY VERDON, editor. *Monasticism and the Arts*. Assisted by JOHN DALLY. Foreword by JOHN W. COOK. Syracuse: Syracuse University Press. 1984. Pp. ix, 354. Cloth \$34.95, paper \$16.95.

This book is a collection of essays that were originally presented at a 1980 conference sponsored by Yale University and St. Anselm's Abbey as part of the celebration of the 1500th birthday of St. Benedict. Skillfully edited and beautifully introduced by Timothy Gregory Verdon, all the contributions are of high quality, most are provocative, and a few break new ground in the study of Christian art. The subject matter varies widely, but the essays generally fall into three categories: the historical ambience of monastic life and art, specific works of art, and monastic influences on art outside the cloister.

Although it is impossible to discuss all the articles, I want to comment on several that I found to be

particularly illuminating. William Loerke's study of two sixth-century mosaics of the Transfiguration, one monastic (St. Catherine's on Mt. Sinai) and one not (S. Apollinare in Classe), suggests that the experiential dimension of monastic culture so often associated with the High Middle Ages can be found quite early in the tradition of monastic art. Jean Leclercq suggests that the monks of the Middle Ages viewed artistic activity as a continuation of the creativity of God. I do not think, however, that he takes into account all medieval monastic art when he describes its essence as neither tragic nor exuberant (p. 76).

Otto von Simson presents two distinct arguments. He suggests, first, that the theology of the Cistercians, and especially of St. Bernard, was an important influence on the evolution of the Gothic style of sculpture and, second, that it can be seen as early as the mid-twelfth century in the central tympanum on the west portal of Chartres Cathedral. Von Simson also examines Bernardine influences on the *Ecclesia* and *Synagoga* figures at Strasbourg and the image of Christ as the Man of Sorrows. He also sees the influence of Bernard's Augustinianism in the proportions of early Cistercian churches. Walter Cahn argues that the Cistercians both adhered to and deviated from their own legislation concerning book illumination. He demonstrates that books given to or purchased by Cistercian houses did not necessarily conform to the legislated restrictions. Unfortunately, the photographs in this essay are not thoroughly described. Sumner Crosby's interesting piece on St. Denis suggests that the emphasis on *regnum* in the facade and *sacerdotium* in the choir was, in Suger's plan, to be linked by a nave whose architecture was reminiscent of St. Peter's in Rome and thus symbolic of the role of the papacy.

Meredith Lillich makes some interesting suggestions in her article on monastic stained glass and indicates the need for art historians to reconsider in light of her research the relationship of monastic spirituality to Cistercian grisaille windows, Suger's use of blue in the windows of St. Denis, and the origins of the watershed in Gothic taste. Although this is a significant piece, Lillich overstates distinctions between the functions of monastic and nonmonastic art when she argues that the former was "not to instruct but to provide stimulus for introspection and encouragements for the meditations of the monks" (p. 213). Are instruction and stimulus for introspection mutually exclusive? She makes some valuable observations about differences between monastic and nonmonastic programs—for example, monks were particularly fond of obscure typologies—but fails to cite sufficient examples. I think immediately of a comparison of the sculptural programs at Vézelay and Autun. In the former

there are many more obscure Old Testament stories than at the nonmonastic St. Lazare at Autun.

Peter Hawkins has contributed a fine piece on Dante's portrayal of Benedict and the contemplatives in the *Paradiso*. He points out that the context of the third *cantica* of the *Commedia* is monastic, because the first and last souls Dante encounters in heaven (Piccarda Donati and St. Bernard) were cloistered contemplatives and heaven itself is described as a cloister in which Christ is abbot (Purg. XXVI:129). Emphasizing the ladder imagery of canto XXII, which Dante borrows from the Rule of St. Benedict, Hawkins suggests that Dante perceived the *Commedia* as itself a ladder that the reader is to climb.

Marvin Eisenberg presents some interesting monastic liturgical allusions in an early painting of Lorenzo Monaco, a Camaldolese monk. William Hood's description and analysis of the Franciscan Sacro Monte of Varallo in northern Italy is especially rich and suggestive. In essence, he argues that this pilgrimage site and others patterned after it are Franciscan "translations" of monastic experiential spirituality for a wide audience. Furthermore, Hood claims that a study of these *sacri monti* challenges art historians to rethink traditional distinctions between medieval and Renaissance. Since this article was written, Hood has created an even richer context for the phenomenon of the *sacro monte* by applying Ewart Cousins's idea of the mysticism of the historical event.

The final article is a survey of developments in American monastic architecture since World War II, and R. Kevin Seasoltz has skillfully approached this subject in light of the tradition of monastic art stretching back a millenium and a half.

Aidan Kavanagh's study of the influence of Evagrius on the Rule of St. Benedict seems misplaced, as does Bennett Hill's article on lay patronage of the abbey of Savigny because he does not relate his topic carefully to the overall purpose of the book. Ruth Steiner's analysis of the music for an office of St. Benedict at Cluny is too technical to fit neatly into this book.

Three areas not addressed seem to me important for a balanced collection of essays on monasticism and the arts. There should be a study of the sculpture of the medieval monastic churches and cloisters of Europe. There is no study of monastic art between the sixteenth century and the end of World War II. Certainly these were not "monastic centuries," but some study of monasticism and the arts in that period would be useful for balance. Most seriously, there is no study of the literature produced by monks. Although the book has some pervasive themes, the contributors could have attempted to address certain general questions more systematically. Nevertheless, this is an excellent volume both

for students of monasticism and those interested in the history of art.

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RICHARD KRAUTHEIMER. *Three Christian Capitals: Topography and Politics*. (Una's Lectures, number 4.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xiv, 167. \$27.50.

Dealing with Rome, Constantinople, and Milan, Richard Krautheimer presents a remarkable synthesis of architectural history and city planning, on the one hand, and social, religious, and political history, on the other. His discussion is unusually well documented by 106 illustrations, including plans, reconstructions, and photos of buildings or their remains, as well as several maps. We may share the author's regret that he could not include a fourth Christian capital, Trier, in this useful study.

Rome is first considered in the early years of Constantine's accession to power, the period in which the Lateran Cathedral (on which Krautheimer is the acknowledged authority) was built. It is the most significant of Constantine's church buildings, followed by the palace church of S. Croce in Gerusalemme and the basilicas associated with the shrines of certain early Christian martyrs. Because of the pagan aristocracy in Rome, Constantine's religious policy remained ambiguous, as evidenced by the placement of all these buildings—which in grandeur were almost all clearly public edifices—on private property, away from the forum, or center of the city. When Krautheimer returns to Rome in his last chapter, it is the papacy that takes the leading role against the last of the pagan traditionalists. The papacy also had to compensate for the relatively remote location of the Lateran Cathedral by erecting new churches and initiating "station services," or papal masses, at these more convenient sites during the liturgical year. This period, beginning with Pope Damasus (366–85) and extending through Sixtus III (432–40) and perhaps Leo I (440–61), is marked by the acceptance of the full classical heritage by the Roman church.

The establishment of Constantinople as "a permanent and Christian capital" (p. 41) for Constantine's empire provides a meaningful contrast to Rome. Here the emperor was free to organize a city with Christian churches at the center, including a commanding location for the church in which he was to be buried surrounded by cenotaphs commemorating the Apostles—with Constantine as the thirteenth. Quite remarkable is the emperor's identification of himself with Christ, the Apostles, and the sun god, Helios, as seen "in the portrait statue atop

the column in his Forum and in the rites established by himself for the consecration of the city in the hippodrome on May 11, 330, and continued into the sixth century" (p. 61).

Milan during the episcopate of St. Ambrose (374–97) is the setting of a complex struggle between the Arian and Nicene parties for control of the church and its property. Here too the buildings, as seen in the context of political and religious developments, embody the dynamics of the struggle. Here too the leading figure, Ambrose, wanted to create a Christian capital in a favored residence of the later emperors, but he was faced with an imperial family sympathetic to the unorthodox Arian creed. Krautheimer develops a strong case that the Church of S. Lorenzo was built to be the cathedral of the anti-Nicene faction close to the palace and circus in the suburbs. Throughout one is struck by the clarity of the argument and the command of a variety of sources.

GREGORY T. ARMSTRONG
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H. R. LOYN. *The Governance of Anglo-Saxon England, 500–1087*. (Governance of England.) Stanford: Stanford University Press. 1984. Pp. xvii, 222. \$27.50.

This is the best and fullest analysis to date of Anglo-Saxon government and is worthy of its distinguished author, H. R. Loyn. The heterogeneous sources—including laws, charters, numismatics, archaeology, and poetry—are successfully conflated, and other scholarly interpretations are given a fair shake. Part 1 surveys the Anglo-Saxon kingdoms to A.D. 899. Loyn concentrates on kingship and its relation to levels of society and to the developing organization of land, law, and wealth and demonstrates the movement from tribal society to "territorial community." Loyn emphasizes Alfred the Great's reign as the "culmination of past trends and as a critical take-off point" (p. xv). Part 2, "Later Anglo-Saxon Kingdoms, 899–1066," pursues these developments in kingship, central government (the secretariat, witenagemot, household, and judicial and financial organization), and local government (shire, hundred and tithing, boroughs, church, private jurisdiction, and military organization). Part 3, "The Norman Conquest," examines the various interpretations of the conquest's impact and defends with convincing shifts of emphasis the now orthodox view that the Norman enterprise used and modified a functioning Anglo-Saxon system.

A brief review cannot do justice to the magisterial handling of sources, the balanced presentation, and the many stimulating suggestions. These range from major matters such as the significance of the heavy concentration of physical and social bases of

monarchy in southern England, the link between developing monarchy and developing towns, and the impact of the tenth-century reform on secular institutions to less vital but no less interesting ones, such as the relation of the cult of Ethelbert of East Anglia to "folk-memory of Mercian unpopularity." Frequent comparison with Continental developments enrich the study. Both the professional Anglo-Saxonist and the novice in the field will finish this work with heightened admiration for Anglo-Saxon achievements in governance.

There are neglected matters, such as the relation of kings to asylum and sanctuary, and questionable assertions, such as that "supernatural sanction followed rather than preceded successful acquisition of power" (p. 13), which contrasts with Frank Stenton's view that "kingship was less a matter of political authority than of descent from ancient gods" among early Germanic tribes (*Anglo-Saxon England*, 2d ed., p. 37). The bibliography and careful index strengthen the book, as over a dozen errors of grammar and printing do not. This important, judicious, masterful book sets a high standard for the new Governance of England series.

WILLIAM A. CHANEY
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PIERRE RICÉ. *Les Carolingiens: Une famille qui fit l'Europe*. Paris: Hachette Littérature. 1983. Pp. 438. 140 fr.

This volume by the eminent French scholar Pierre Ricé tells the story of the Carolingian family and the influence it exerted on Europe from the beginning of the seventh until the end of the tenth century. The book consists of five general sections. The first deals with the origins of the Carolingians as a vital force from about 600 until the year 751, when Pepin III, following the abdication of his brother Carloman, became the sole ruler of the *Regnum Francorum*. The second continues this story from 751 to 814, the years when Pepin became a crowned monarch and his son Charlemagne succeeded him as king and then emperor. The third discusses in some detail the period between 814 and 877, from the accession of Louis the Pious to the death of Charles the Bald. The fourth tells us about the decline of the Carolingians and the last years of this family's authority in France as new European political entities arose, especially the empire of the Ottos of Germany. The book concludes with a brief series of chapters that sums up Carolingian influence on the church, the character of kingship, the economy, and the culture in Western Europe.

Throughout this study Ricé's approach is predominately prosopographical, not only in regard to the Carolingians themselves but also to those Aus-

trian noble families that Gerd Tellenbach and his school have shown were intimately associated with both Carolingian early successes and later failures. He has also provided us throughout with considerable distilled information concerning how the Carolingians and these associated nobles dealt with church establishments in the empire or in the particular regions these nobles dominated. This is done so extensively that one might almost come to think of this volume, which is supplemented by some thirty-four genealogical tables of the leading families, as a prosopographical history of the European nobility during four centuries.

This is, of course, very important, and in its totality Ricé's book will no doubt long be invaluable to those who need to disentangle the complex family relationships of those who controlled much of Europe for so many centuries. But it also leads to some important questions about the possible limitations of the prosopographical method that the author has employed.

On the basis of this approach, so beautifully elucidated for us, can we really believe that the Carolingians created Europe, as the author states again and again? Or were other factors at work? Can we, for example, view the role of the church and the papacy as secondary to that of the Carolingians and their noble allies? Or was Christopher Dawson not closer to the truth than Ricé in this matter?

And can we really explain Europe's genesis without more attention to those parts of the Continent beyond Carolingian control, like the Byzantine empire, the British Isles, most of northern Spain, and Scandinavia, or to those cultural areas, like southern France, Italy, or Eastern Europe, over which Carolingian control had all but disappeared? Must we not also consider agrarian innovators in the countryside noted by Lynn White and maritime advances, especially in Northern Europe?

Also, what role, if any, did the Carolingians play in the appearance of a new noble military class throughout much of tenth-century Europe, as castles multiplied and vitally changed the political and social landscape of the Latin West? And what of the middle class and its origins, which Pirenne and his school have emphasized? European culture certainly had its roots in much more than family interests and authority. This fine book tells us a great deal of what we need to know about the Carolingians and the noble houses associated with their rise and fall. But we must be careful not to accept fully the second part of Ricé's title, "A Family That Made Europe." For Europe, even in this early period, was always much more than the Carolingians.

ARCHIBALD R. LEWIS
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JOHN C. SHIDELER. *A Medieval Catalan Noble Family: The Montcadas, 1000–1230*. (Publications of the UCLA Center for Medieval and Renaissance Studies, number 20.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xxi, 252. \$34.50.

The Montcada family first became prominent during the eleventh-century aristocratic upheavals against the counts of Barcelona. Although generally loyal to the embattled counts, the Montcadas benefited from the independence won by other emerging noble lineages. During the eleventh century the Montcadas assembled possessions in the territory of Barcelona and in the districts of Vallès, Osona, and Bages. The institution of primogeniture assured the continuity of this patrimony and preserved the identity of the family through succeeding generations.

In 1117 the last of these original Montcadas, Beatriu, married Guillem Ramon, seneschal of the count of Barcelona, and the children of this marriage would take their mother's name, Montcada. The seneschals were a minor noble family and their hereditary but ill-defined office was obtained in 1068. John C. Shideler's major accomplishment lies in showing that the family of the seneschals was separate from the Montcadas before the marriage of Beatriu and Guillem Ramon. He has thus vindicated the opinion of the eighteenth-century *marqués de Mondéjar*, while demonstrating the error of more recent historians who confused the Montcadas with the early seneschals. In doing so, Shideler has not only revealed the true origins of the Montcadas but has also revised our understanding of the seneschal's office and the power of the Catalan nobility.

Historians confused the two families by exaggerating the status of the early seneschals. That the seneschals would take by marriage the name of a noble, nonofficial family seems illogical if their title already carried great prestige. In unravelling a complex genealogical puzzle, Shideler has made apparent the superior reputation of the Montcadas (whose strength was based on castles, land, and ecclesiastical office) over the comparatively upstart seneschals, whose ascent was assured by alliance with the older house whose name they adopted. The merging of the families was a joining of two forms of power: official and territorial. The Montcadas, in the century that followed, would play the roles of both political counselors and violently independent nobles.

The latter part of the book, treating the period 1117–1230, is more episodic. It follows the careers of the Montcadas through the expansion of Catalonia and its several crises. By the time of the heroic death of Guillem de Montcada [III] at the invasion of Mallorca, members of the family held important

interests in Tortosa and Lleida and were viscounts of Béarn. An outline of the Montcada's lordship is given, contrasting their estate administration in Old Catalonia with the jurisdictional rights they held in the more recently conquered areas. Shideler offers few sweeping conclusions, but he makes interesting suggestions concerning the causes of the growing indebtedness of the Montcadas and the vagaries of their peculiar relationship to the count-kings.

Shideler's assiduous research and respect for his sources are exemplary. He is cautious in his generalization, yet his work should attract the interest not only of historians of Catalonia but also of students of the difficult subject of the medieval nobility.

PAUL FREEDMAN
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H. E. J. COWDREY. *The Age of Abbot Desiderius: Montecassino, the Papacy, and the Normans in the Eleventh and Early Twelfth Centuries*. New York: Clarendon Press of Oxford University Press. 1983. Pp. xli, 300. \$45.00.

H. E. J. Cowdrey set out in this book to study the relationship between Montecassino and the reform papacy during the age of its greatest abbot, Desiderius, who succeeded Pope Gregory VII under the name Victor III. The subject, difficult and at times controversial, is of considerable importance. The sources for the history of the monastery in this period pose numerous problems, not least of which is that spirit of particularism exemplified in the work of Peter the Deacon. Desiderius himself remains at the center of disputes regarding his excommunication by Gregory VII and the validity of his election in Rome in 1086. But there is no reason to doubt his greatness as an abbot. His accomplishments in developing the *terra Sancti Benedicti* and reconstructing much of the abbey (including building a new basilica) are sufficient to establish his distinguished place in its history. But some historians have limited his role only to these attainments and argued that he placed the interests of Montecassino before those of the papacy and the reform movement. In particular, his relations with the Normans of southern Italy have been viewed in these terms. He has often been regarded as a man of narrow vision in a world that called for more. To counterbalance this interpretation, Cowdrey discusses the representation of Desiderius qua Victor in the Lateran frescoes of the 1120s and 1130s as an authentic and important member of the line of reform popes that included Gregory VII and Urban II. Such a position challenges the dominant interpretation of Desiderius, even that given by his chief proponents.

Had Cowdrey contented himself with revising the interpretation of Desiderius, his achievement would have been notable. But he has done much more. He has studied the total relationship between Montecassino and the reform papacy in this period and has succeeded in showing not merely the importance of this great monastic center to the popes but also the evolution of its own vision of reform. Building on work by scholars like Herbert Bloch and Ernst Kitzinger, Cowdrey is able to trace the literary elements of the concept of the renewal of the early church that were iconographically expressed in the mosaics of Montecassino and Salerno in the age of Desiderius and that linked these monasteries to the reform movement. The sophisticated presentation of the embrace of reform ideas at Montecassino is one of this work's most important contributions. Scarcely less important is the manner in which the author details the role of Desiderius and other abbots in developing the economic foundations of the monastery. In my opinion, Cowdrey does not go far enough in connecting these systematic efforts to fundamental concerns of the reform movement, but his presentation and analysis of the evidence is excellent. He is also at pains to show that Montecassino's complex relations with the Norman and Lombard rulers of southern Italy served not only the interests of the monastery but often those of the reform papacy as well. What emerges from this study is not a tendentious denial of Desiderius's and Montecassino's commitment to policies aimed at glorifying the cradle of Benedictine monasticism, but a frank recognition that their commitment to the reform movement had a distinctive flavor because of that commitment.

The difficulty of the problems with which the author is concerned raises the probability of some disagreement. For example, I am not fully convinced by his argument that Archbishop Hugh of Lyons was inadequately informed concerning events in Rome to know whether Gregory VII had excommunicated Desiderius. On the whole, I do not find his effort to disprove the excommunication convincing. Even if Desiderius was excommunicated for some time during this volatile period, that does not disqualify him from membership in the Gregorian party. But Cowdrey knows that the evidence is insufficient to settle this matter, and he handles it with an integrity that gives the reader confidence in his judgment.

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KLAUS HERBERS. *Der Jakobuskult des 12. Jahrhunderts und der "Liber Sancti Jacobi": Studien über das Verhältnis zwischen Religion und Gesellschaft im hohen Mittelalter.* (Historische Forschungen, number 7.) Wiesbaden:

Franz Steiner, for the Historische Kommission der Akademie der Wissenschaften und der Literatur. 1984. Pp. xii, 251. DM 89.

Klaus Herbers proposes to use the cult of St. James of Compostela in the twelfth century to explore the relationship between religion and society. Although there are appropriate references to the differences between popular and scholarly religious beliefs and to the history of *mentalités*, Herbers's book can best be described as an introduction to a critical edition of the *Liber Sancti Jacobi* (although Herbers does not indicate whether he is preparing such an edition). The *Liber*, written in the mid-twelfth century by an anonymous author who ascribed his work to Pope Calixtus II, is a compilation of five different parts that were subjected to later additions and revisions: the liturgy and miracles of St. James, the legend of the translation of his body to Spain, an account of Charlemagne's Spanish campaign attributed to Archbishop Turpin of Rheims, and a guide for pilgrims. The fourth part, which includes the story of Roland's death at Roncesvalles, has attracted the attention of scholars interested in the origins of the *chansons de geste*.

After discussing the origins of the legend of St. James, the compilation of the *Liber*, and the oldest manuscript (the *Codex Calixtinus*), Herbers focuses on three problems in his textual exegesis: the ecclesiastical position of the compiler, the knightly milieu of the cult, and the spiritual and economic aspects of the pilgrimage to Santiago. Herbers thinks that the author of the *Liber* was a regular canon who perceived a link between the apostolic life of a canon devoted to preaching and the Christian knight who was prepared to die in a holy war against the infidels. St. James, the first apostle to be martyred, was a model for both.

This is fine as far as it goes, but I doubt that the French medievalists whom Herbers cites as his methodological authorities would have limited their discussion of the connection between religion and society as seen in the cult of St. James to the analysis of a single hagiographical text. Surely, the topic also requires an investigation of the spatial and social diffusion of the cult over time. For example, before leaving for Santiago in 1147-67, Frederick III of Haunsberg gave a farmstead to the Abbey of St. Peter in Salzburg. To say that the cult appealed to knights does not explain why a nobleman who lived in the eastern Alps decided to travel to the northwestern corner of Spain rather than to Rome or Jerusalem, especially because it is doubtful that French chivalric culture had reached southeastern Germany by the mid-twelfth century. Similarly, an examination of the economic impact of the cult cannot simply cite references in the *Liber* to pilgrims bringing gifts to Compostela. To execute Herbers's

proposed study, it would be necessary to compare the canon's propaganda with information about the cult's appeal provided by other sources. In short, this is a good introduction to the content of a difficult text, but it is not the social history of religion that its subtitle promises.

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GÉRARD SIVÉRY. *L'Économie du royaume de France au siècle de Saint Louis, vers 1180-vers 1315*. Lille: Presses Universitaires de Lille. 1984. Pp. 339. 250 fr.

After publishing several fundamental studies on the economy and society of late medieval Hainaut, Gérard Sivéry has turned his attention to the thirteenth century in two works of synthesis encompassing the entire kingdom of France: *Saint Louis et son siècle* (1983) and the present, more original, book on the economy. His thesis is that economic crises similar to the well-documented ones of the fourteenth century occurred as early as the thirteenth century and were the consequence of a new type of economy that first emerged in several regions of France around 1200. Although large parts of central and western France remained mired in what he calls a "traditional economy" of self-sufficiency and immobility, the more urbanized areas of northern France and the Mediterranean coast developed a "new economy" based on competition, the production of goods for markets, and rapid shifts in supply, demand, and networks of distribution, all of which resulted in the appearance of economic cycles. Sivéry argues, in effect, that the standard depiction of the thirteenth century as a period of general and sustained economic growth ignores the existence of severe short-term crises and regional differences and further misrepresents the nature of economic activity at that time.

In one sense, Sivéry's model of a capitalistic thirteenth century revives Henri Pirenne's preoccupation with urban production and long-distance trade as the motor of the medieval economy. But Sivéry attempts a more sophisticated analysis that links grain prices, land rents, cloth production, interest rates, and demographic trends. The core of his argument is that cities began to dominate the French economy precisely because their dependence on imported foodstuffs and on exports susceptible to sudden shifts in markets and routes made them highly responsive to slight economic changes. Growth, crisis, and decline thus became permanent features of the new economy. Regional differences remained, however; for example, the decline of the Flemish cloth industry from the 1260s coincided with the extension of textile production to new urban centers, where large quantities of lower-quality

cloth met the needs of rapidly increasing populations.

This book is a timely reminder of how little we know about the economy of medieval France, despite the magisterial regional studies of the 1960s and 1970s. The chapters on the Flemish cities, cloth production, and international trade are the best since they rest on extensive prior scholarship, whereas those on price movements and urban economies outside Flanders remain fragile in the absence of solid studies in those areas. The book is at once stimulating and frustrating to read—stimulating because it considerably enlarges a field of inquiry in need of fresh thinking and research; frustrating because important conclusions are drawn from very thin evidence, despite the author's cautionary remarks on methodology. Moreover, there are an inordinate number of typographical errors and incomplete bibliographical citations. In sum, this is a provocative questionnaire for further research, but it is a book to be used with caution by anyone seeking an introduction to the economy of thirteenth-century France.

THEODORE EVERGATES
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MICHAEL BURLEIGH. *Prussian Society and the German Order: An Aristocratic Corporation in Crisis, c. 1410-1466*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1984. Pp. x, 207. \$39.50.

The collapse of the German Order (Teutonic Knights) as rulers of a larger crusader state between the Holy Roman Empire and Poland-Lithuania has long been grist for many nationalistic mills, both German and Slavic. It is to Michael Burleigh's credit that he has brought a calm clarity to the subject of the eclipse of the order in Prussia, which will interest students both of German history in particular and late medieval society in general. His study is both a continuation and an intensification of Francis L. Carsten's *The Origins of Prussia* (1954) and, to a lesser degree, of Carsten's *Princes and Parliaments in Germany* (1963). Burleigh's mission is twofold: to describe the evolution of the German Order in Prussia from the disaster of Tannenberg in 1410 to the loss of its monumental headquarters of the Marienburg and to trace the development of the opposition movement of lay nobles and townsmen called the Prussian Union. Whereas traditional German historiography has portrayed this process as yet another betrayal of *Deutschtum* in the face of *Slaventum*, contemporaries saw the matter very differently. The order was an alien corporation, closed to native Prussians of all social ranks and staffed mostly by nobles from southern and Rhenish Germany. Its

mission to fight for Christianity against pagan Prussians, Livonians, and Lithuanians had lost even a specious justification with the conversion of the last Lithuanian prince and the unification of Poland and Lithuania in 1386.

Even if the order had difficulty justifying itself in terms of its original mission, it probably could have maintained itself as the dominant force in Prussia for an indefinite period of time if it had not been so abysmally led. The Prussian Union came into existence largely because the nobles and townsmen of Prussia did not trust the order, which resorted to extortion, murder, and strongarm tactics when it could get away with them. The territorial losses that followed Tannenberg went together with dramatic increases in taxation to fund an increasingly forlorn defense against Poland. In the end, the Prussian Union would prove to be the means by which Prussia became what it would remain until the seventeenth century, a German-speaking province under the Polish crown.

Burleigh has based his study solidly on archival sources, and the result is a convincing and useful portrait of one of the most interesting examples of decaying petty feudality. As the author underlines repeatedly, the nobility of Germany regarded the German Order primarily as a fitting and proper place to put their sons, and this social function took precedent over any "mission" that might once have brought the knights of the black cross to the shores of the Baltic. In Prussia itself, the colonial days had passed by the fifteenth century, and the inhabitants were ready and willing to find some way of living as a normal part of Eastern Europe. The result was a reduction of the Teutonic Knights and the introduction of a lay-estate society in Prussia analogous to that found in the rest of Eastern Europe.

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JENS E. OLESEN. *Unionskrige og staendersamfund: Bidrag til nordens Historie i Kristian I's regeringstid, 1450-1481* [The War of Union and the Estates Society: A Contribution to Scandinavian History in the Reign of Kristian I, 1450-81]. (Skrifter Udgivet af Jysk Selskab for Historie, number 40.) Aarhus: Universitetsforlaget. 1983. Pp. 534.

This scholarly and well-documented book by Jens E. Olesen on the troubles of the Kalmar Union is a sequel to his earlier work *Rigsråd—Kongemagt—Union* (Rigsråd, Royal Power, and Union), which covered the period 1434-49. Rounding out his work, Olesen has also published a number of shorter articles on such topics as Erik of Pomerania, Duke Adolf VIII of Schleswig, and fifteenth-century

Lithuania. These articles provide an excellent background for this latest, important work.

The thread running through this book is the idea of the Swedish nation as opposed to the Scandinavian union born in 1387. The conflict is clouded by fights among the aristocratic families sitting on the councils of the realm of Denmark-Norway and of Sweden (including Finland). When the Bavarian-born king Kristoffer (1439-48) died, his successor to the Danish throne was Kristian I (1448-81), the first of the Oldenburg line. A popular uprising in Sweden, led by mineowner Engelbrekt Engelbrektsen, spread to Norway in protest to royal rule through imported German and Danish administrators. Kristian's right to Swedish fealty was challenged by the aristocratic Karl Knutsson Bonde who, with an army to back him, was elected king in Sweden and Norway. This book details the intermittent wars Kristian fought to preserve the Kalmar Union.

Olesen points out that the initial phase of the struggle was over Kristian's right to the Kalmar Union throne, compromised initially by Karl Knutsson's withholding of the Swedish "morning gift" of estates to Queen Dorothea, the widow of Kristoffer and later the spouse of Kristian I. The basic issue was the struggle between an unsharing and uncompromising king and a Swedish aristocracy wanting constitutional checks on the monarchy, which was a new political idea in Sweden. Olesen offers insight not only into the intrigues and clashes among the aristocratic families but also into Danish efforts to isolate Karl Knutsson through manipulation of the Wends, the Livonians, the Hanseatic League, and the Teutonic Order. Olesen details the hostility of the large, initially pro-Danish Thott family toward the Oxenstierna-Vasa faction. Momentary expediency and intermarriage led to cooperation between noble families and the king, but relations gradually chilled as other factors—such as the spendthrift Kristian's acceleration of maturity on secured loans and interest, both in Denmark and Sweden—hit the "pocket-book nerve" of aristocratic families and finally ended loyalties to the union king.

Kristian's troubles were not limited to Sweden. Within Denmark a half-dozen mighty aristocratic families strove to curb the Oldenburg king's hunger for power and money, and they did not hesitate to cross the sound to get allies.

Olesen shows that, if Kristian had trouble getting his Dano-Swedish throne, so also did Karl Knutsson Bonde in Sweden. And the Hanse cities meddled and mediated for their own gain. With the death of Karl Knutsson in May 1470, the Swedish council of the realm chose another aristocrat, Sten Sture, as regent to repel the expected invasion by Kristian, who threatened to take advantage of the instability in succession. The king's unwillingness to negotiate

with the Thott-Axelsson family about Danish-held Gotland made battle inevitable. At Brunkeberg in the suburbs of Stockholm on October 10, 1471, Kristian met defeat and thereby lost any chance of gaining the Swedish throne by military means; only an aristocratic constitutional compromise could gain him the throne. Kristian's failure to pay his bills alienated the "good men" of Stockholm, who in turn swayed the estates. The Strängnäs meeting in late 1477, Olesen explains, dashed Kristian's hopes of acceptance as king; the Swedish estates, led by Sten Sture, refused to approve as king one who avoided recognizing the limitations imposed by them. The support of the pro-Danish Axelsson-Thott family evaporated under peer pressure. The death of Kristian I in 1481 left the problem to his successor. Hans was crowned in 1497, but Gustav Eriksson Vasa undid it all in 1523. Sweden has been independent ever since.

Olesen is so thorough that he lists twenty-four pages of sources and sixty-three pages of notes. The concluding summary is in Danish, but there is no German or English condensation.

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MODERN EUROPE

BERNADOTTE E. SCHMITT and HAROLD C. VEDELER. *The World in the Crucible, 1914-1919*. (Rise of Modern Europe.) New York: Harper and Row. 1984. Pp. xvii, 553. Cloth \$20.00, paper \$9.95.

The first volumes in the Rise of Modern Europe series, conceived and edited by the late William L. Langer, appeared in 1934. In the original planning, which took place little more than a decade after the Great War, this volume was to be the last in the series, and it was accorded the shortest time span of all the volumes, five years. The late Bernadotte E. Schmitt, who in 1930 published his prize-winning opus on the origins of the war, was commissioned to do the book, and he gave it the title *The World in the Crucible*. The general thesis—that the war was a revolutionary event—was thus set. Schmitt died in 1968, having collected much material but having produced only several draft chapters on the military war. Harold C. Vedeler, who received his Ph.D. in 1933 but then spent most of his career in the American foreign service, was enlisted by Langer to complete the work.

One has to wonder what Langer would have thought of the end product. His editorial preface, printed in every volume in the series since 1934 but significantly omitted in this one, spoke of "the current broad conception of the scope of history"

according to which a historian must "attempt to go beyond a merely political-military narrative, and to lay stress upon social, religious, scientific, and artistic developments." To date, every other author in the series has paid serious attention to this appeal. This volume does not.

With the exception of a few brief sections, not even chapters, that deal with broad social, economic, and intellectual developments, the book is devoted almost exclusively to a narrative of the military and naval war and then to an account of political events in Central and Eastern Europe at the end of the war. Much of this is admittedly done very well. The capsule narratives of the major battles are excellent, and the attention paid to "the peripheral war" on the southern flank is welcome. The prose, moreover, has an old-school clarity that makes it a joy to read. On its own the book has merits, but it appears in a series that has set very high and different standards, and on this score it falls short.

Vedeler points out in his concluding remarks that, if the war had any positive features, one was its democratizing effect. In those four years, more intensely than in any other four, traditional hierarchical authority suffered a momentous defeat and the democratic impulse received a corresponding boost. In short, he appreciates that there are, in fact, two stories to be told about the war, one about the collapse of the old order and the other about the emergence of the new. Yet the authors have concentrated primarily on the former tale. There is little to be found on the emotions, frustrations, and other moods of the fighting men, let alone of the masses of civilians. There is little to explain how the war could continue for more than four years. It of course could continue only if people were willing to fight. Why were they willing? General assertions—"The Somme was the burial site of the old German army, with its superb qualities of steadfastness and fighting spirit" (p. 136)—are frequent, but they are rarely documented adequately or persuasively. They remain assertions. The armistice, we are told, "brought a delirium of ecstasy to the peoples of the Allied countries and a feeling of relief to the German population" (p. 295). Ecstasy? Relief? One wonders. The evidence suggests something closer to manic depression and hysterical anxiety as the more common states of mind. At any rate, without proper support such statements have an air of literary device rather than reliable conclusion.

In the end the volume embodies a paradox. It argues that revolution was the central feature of the Great War and that the modern world was transformed irreparably by the cataclysm. Yet the authors show little awareness that the methods of the historian had to undergo a comparable revolution in relation to nineteenth-century models if events in this century were to be explained in any convincing

manner. In setting guidelines for the series, Langer was acutely aware of this issue. "To trace the process of social transformation and to recapture what Wordsworth called 'the still, sad music of humanity,'" he wrote in 1969 in his own contribution to the series, "is far more difficult than to narrate the story of court intrigue, or trace the evolutions of political struggle, or recapture the history of military exploits." With every respect for the obviously enormous labors already evident in this book, one is forced to say that despite Langer's vision the authors took the easier route.

MODRIS EKSTEINS
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CAROLE FINK. *The Genoa Conference: European Diplomacy, 1921-1922*. Chapel Hill: University of North Carolina Press. 1984. Pp. xviii, 365. \$36.00.

It is difficult to write a good book about a conference that failed miserably, but Carole Fink has done just that. The Genoa conference of April-May 1922 was the last of the postwar summit conferences and the first and only attempt to solve the Russian problem in a universal conference. It was the swan song of David Lloyd George and Walther Rathenau, and it spawned the Treaty of Rapallo. There has long been a need for a book on Genoa, and Carole Fink has admirably filled the gap. Her study is based on impressive research in British, French, German, Italian, Belgian, Austrian, American, and Canadian archives and on a host of private papers. The book is well written and contains a fine bibliography.

Despite the intensive research, there are no major surprises about Genoa, except perhaps the disorganization and haphazard antics of the German delegation. But the lack of revelations does not mean that the book is disappointing. What Fink has done is to roll out a rich tapestry of the diplomacy of the first "normal" year after the Great War, 1922. I have seldom read a clearer, more illuminating account of the day-to-day maneuvers of diplomats at a conference. Besides the conference Fink discusses all sorts of intriguing themes: one of the last moderate Italian governments vainly gambled that Lloyd George would solve its problems before Mussolini did; Georgi Chicherin and Maxim Litvinov vied for success; Edouard Beneš began to practice his subtleties; the League of Nations painfully tried to get someone to use its services. Anyone seeking to understand the character of postwar Europe will profit from this study.

The most important contribution of the book is what we learn about Lloyd George and Anglo-French disunity. Although the author is sympathetic to Lloyd George's idealism, her analysis of his poli-

cies is damning. As she aptly says, the essence of Genoa was that "Lloyd George sought primarily to bring Russia back into Europe, using Germany as an accessory and accomplice" (p. 304). In 1922 this amounted to trying to solve the Russian problem before the German problem was solved. Lloyd George and his amateurish entourage simply did not realize how complicated and volatile these problems were. Nor did they realize that Britain did not have the financial and political power to make Russia, Germany, France, and the United States cooperate.

Above all, Lloyd George failed because he consistently refused to take the problem of French security against Germany seriously. The French had their own shortcomings, but they had a case, especially after the British had walked away from their Guarantee Treaty with France. Lloyd George assumed that he could hector the French into conceding anything. He succeeded rather well with the top French delegate in Genoa, Louis Barthou, but it was child's play for Raymond Poincaré, the premier, to stay in Paris and disavow Barthou's concessions. Fink concludes that Lloyd George, "who knew Britain could do little to restore peace in Europe without France's full cooperation, decided to exaggerate France's dependency and ignore Britain's" (p. 276).

WILLIAM E. SCOTT
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MONTY NOAM PENKOWER. *The Jews Were Expendable: Free World Diplomacy and the Holocaust*. Urbana: University of Illinois Press. 1983. Pp. x, 429. \$21.95.

In *The War against the Jews, 1933-1945* (1975) Lucy Dawidowicz divided the study of the Holocaust into three spheres: the perpetrators, the victims, and the bystanders, or potential rescuers. The first and second groups have been exhaustively examined; the third, though last to undergo scholarly investigation, has now become a major subject of historical inquiry.

Monty Noam Penkower's book consists of nine essays and an epilogue, some of which have been published in earlier versions. Organized chronologically and unified by the themes of Allied indifference and Jewish powerlessness, the book examines the long-delayed establishment of an Allied-Jewish army, the thwarted voyages of the *Patria* and the *Atlantic* to British Palestine, Allied and neutral inaction on receiving irrefutable evidence in 1942 of Germany's implementation of a "Final Solution" to the Jewish question, the abortive Bermuda Conference on refugees in 1943, the belated creation of the U.S. War Refugee Board, the Allies' failure to support Jewish rescue attempts in the Balkans, the vain efforts to save a half million Hungarian Jews,

the noncooperative relations between the International Red Cross and the World Jewish Congress, and the desperate, final-hour negotiations between Jews and Nazis in the spring of 1945.

Penkower conducted over fifty interviews and examined newly opened private papers, the archives of the Allied governments and the major Jewish organizations, as well as all standard secondary sources. He narrates a history of persistent, courageous, and selfless rescue attempts; frustrating delays, obstruction, and anti-Semitism in official Allied circles; and the mounting number of Jews slaughtered between 1942 and 1945. He argues that a "psychological hindrance" prevented both the victims and their potential rescuers from acknowledging the full dimensions of Nazi atrocities: traditional Western liberalism and reaction against the atrocity propaganda of World War I explain both the widespread incredulity and the failure to respond to the Nazis' obsessive, mechanical, and sweeping annihilation of a single people.

Although targeted for destruction, the Jews were not recognized by the West as a separate people, a potential ally against the Third Reich, or the object of specific rescue plans. Penkower criticizes London's adherence to the "draconian" White Paper and questions the Allies' inability to save Jewish victims. Massive food relief went to occupied Greece but not to the Polish ghettos; Yugoslav and Greek refugees were sheltered in Middle Eastern camps, but only a handful of Jews reached safety in North Africa; despite logistical and bureaucratic complications, the United States transported four hundred thirty thousand German prisoners of war to America, but no ships were available to save Jews fleeing from extermination; and, in August and September 1944, the West gave "far higher priority" to supporting the Polish uprising in Warsaw than to destroying the death machines at Auschwitz.

In his fervid, detailed indictment of Free World policy, Penkower occasionally fails to distinguish between the single-minded enormity of Nazi crimes and the West's irresolute responses. His work parallels and supplements David Wyman's *The Abandonment of the Jews: America and the Holocaust, 1941-1945* (1984). Allied policy in World War II was undoubtedly based more on national self-interest than on concern for an endangered people, and Allied leaders appreciated the limits of their resources. Penkower criticizes the Free World for failing to halt or mitigate an unprecedented, irrational act of destruction and for concentrating primarily on destroying and punishing the perpetrators.

CAROLE FINK
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DAVID HAPGOOD and DAVID RICHARDSON. *Monte Cassino*. New York: Congdon and Weed. 1984. Pp. 269. \$17.95.

"One day," wrote Abbott Gregorio Diamoro amid the clash of battle around his abbey, "the world will learn the truth about what happened at Monte Cassino." David Hapgood and David Richardson have attempted to fulfill the old monk's prophecy in this detailed account of the events surrounding the Allied bombing of the Benedictine monastery on February 15, 1944. The bombing has been variously characterized since as a military necessity, a colossal blunder, and even a war crime. The evidence presented here indicates that "blunder" is surely the most apt characterization.

Given that the monastery stood on a mountain top squarely athwart the route of advance of General Mark Clark's Fifth Army in its drive on Rome and that Clark's design was to break the German defenses by frontal assault, destruction of the abbey was almost inevitable. To Allied soldiers, stalled on the approaches to Monte Cassino, it seemed inconceivable that evil Germans were not using the abbey as a defensive strongpoint. In truth, there were no German soldiers in the monastery, and the Germans did make a creditable effort to preserve this cultural monument and its art treasures. Therein lies much of the tragedy of the bombing of Monte Cassino.

Two German officers, one at least sincerely interested in cultural materials (the other may have been motivated by a desire to furnish art for Goering's collection) managed to have the abbey's art treasures and most of its monks evacuated to places of safety. The German commander in the area, General Frido von Senger, a consummate professional soldier but no Nazi, did his best within the limits of his own ideas of military necessity to preserve the monastery. But he did permit placement of machine guns and ammunition bunkers close to the walls of the buildings. No German soldier, however, occupied the abbey itself, nor was it used as an observation post. About two thousand civilian refugees did join the monks in what they thought was a sanctuary from the raging battle.

When New Zealand General Bernard Freyburg assumed from American units the task of storming the heights of Monte Cassino, he insisted that the abbey must be bombed before his Commonwealth troops could advance. Clark and other American commanders were opposed, but the theater commander, British General Sir Harold Alexander, believed it politically wise to support Freyburg. The American Air Force commander, General Ira Eaker, decided to make the bombing a massive assault designed to show the Air Force's capability to support an infantry advance. So the abbey was reduced to ruins by American bombers. The abbott

and the monks survived, but many civilian refugees were killed or wounded. The Germans suffered no losses and promptly moved into the ruins of the abbey to establish a practically impregnable defense—so much for military necessity.

The authors have told this story of a tragic blunder in an objective manner with grace and literary aplomb, using to good advantage the diaries of the monks and the records and recollections of German officers involved. The book should appeal to the general reader as well as the specialist on World War II.

ROBERT W. COAKLEY
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ALAN S. MILWARD. *The Reconstruction of Western Europe, 1945–51*. Berkeley and Los Angeles: University of California Press. 1984. Pp. xxi, 527. \$38.50.

This massive study asks why reconstruction after World War II was more successful than after World War I. How did the postwar boom start? Why did it continue despite adverse circumstances and then change? How did integration succeed and prolong the boom? Seeking the answers, Alan S. Milward combed records of fourteen nations and eight international entities. As he acknowledges, evolution of national policies remains to be studied, but his monumental research at the international level is unlikely to be repeated soon.

Milward finds some answers in Anglo-French resistance to American demands for sweeping integration. The Marshall Plan derived not from economic disaster, impending communism, or idealism but from American strategic self-interest. It averted a payments crisis but accomplished little else. The 1945–67 boom arose from West European expansionist policies, continued owing to Marshall aid, and lasted thanks to Europe's own integrationist devices. It survived the 1949 American recession because of intra-European trade arising from German recovery and then built on France's limited economic solutions to the Franco-German problem. Facing Anglo-American hostility to the fragmentation of Germany and American pressure for integration, France conceived Franco-German economic association and timed it well. The bases of effectual reconstruction and a long boom were the European Payments Union, the Coal and Steel Community, the Common Agricultural Policy, and, finally, the Common Market. Europe created its own peace settlement, combining nation-states with limited integration based on national interests and the art of the possible.

Milward's comparison of the two postwar periods suffers from superficiality and facile assumptions about the first, but his conclusions about his topic

are convincing and overwhelmingly documented with statistics. Much data should have been relegated to appendixes. Milward announces a political and economic history of European reconstruction, but, although he knows the key decisions were political, his focus is economic and scants other factors. Surprise at French hopes for German fragmentation and belief that Belgium might have accepted a French-dominated customs union disregard historic fears. Some statistical manipulations are unfortunate: knowing European electorates wanted more to eat and assuming both equal food distribution in Europe and equal caloric content in all foods, Milward demonstrates that in 1949 some governments "could have" continued capital goods import without Marshall aid by holding rations to 1947 levels.

Nonetheless, this is a major work despite its lack of readability. Milward assumes expert knowledge and plunges into mid-1947 without background or much explanation thereafter. His prose style, punctuation, lack of transitions between paragraphs, ambiguity about dates and excursions into past and future, failure to define terms, misleading chapter titles and opening statements, streams of statistics, and very inadequate index frustrate the reader, as does carelessness: Molotov evidently left a purely Anglo-French meeting debating an invitation to him. How the Payments Union came about is not explained, and the chapter on the Schuman Plan is discursive and confusing.

Milward suggests his conclusions be read first. This is essential to follow the thread through data and digressions. He assumes few will read further. Those who do will be rewarded by an important, thought-provoking book.

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HORST LADEMACHER, editor. *Gewerkschaften im Ost-West Konflikt: Die Politik der American Federation of Labor im Europa der Nachkriegszeit*. (Kasseler Forschungen zur Zeitgeschichte, number 1.) Melsungen: Schwartz. 1982. Pp. 291. DM 42.

This mildly revisionist book about the American Federation of Labor (AFL) in Europe is the first volume in a new series from the University of Kassel. Horst Lademacher provides a highly detailed essay, "Confrontation at the Beginning of the East-West Conflict, Activities in the Western Zones of Germany." Writing the volume's most elegant prose, he reveals that the AFL's strong anticommunism underlay its wishes for the emergence of independent, democratic labor unions in postwar Germany as well as its material aid to German unions, its lobbying with U.S. officialdom, and its bitter oppo-

sition to the World Federation of Trade Unions, which it saw as a Soviet tool.

Margherita Zander's "The Weakness of Incomplete Unity: Italy" recounts the postwar revival of unions, which was marred by southern Italy's economic weakness and the deep divisions between Roman Catholics and the left. She expertly chronicles the bloody conflicts between left and right in Italy in the context of the growing Cold War. As presented here, the AFL was on the fringes rather than at the heart of matters.

Henk Reitsma displays a fine sense of irony in "Mixed Hope and Losses: Activities and Results in France," the tale of the *Confédération Générale du Travail* and its split. One or two weaknesses in argument do not harm his case that the AFL failed to reach its goal of a unified, anticommunist union thanks to ignorance of French labor traditions and conditions.

Angelos Avgoustidis misnames his essay "Between Left and Right: Decision without Discord: Greece." Discord was in oversupply in postwar Greece, especially in the struggle, which the right won, to create the major union, which Avgoustidis rightly calls an "anti-democratic fortress" (p. 228). He clearly explains the AFL's collaboration with the State Department to save Greece from communism and notes the AFL's desire for democratic government and unions, a wish not fulfilled amid rightist repression.

Karl Heinz Pütz offers "Some Thoughts on AFL/CIO Foreign Policy," a discursive essay in clear but stilted English. He explores the AFL's approaches to foreign policy: anticommunism, policies made and executed by a small elite, a tendency to go along with government and even business views (mostly successful when operating within an American consensus), and the pursuit of self-interest that had been shifting from free trade to protectionism.

All the essays are based on wide research in archives, periodicals, and secondary works. The arguments are generally well wrought, and the authors provide more than ample evidence for their conclusions, adding to our knowledge of American activities in Europe early in the Cold War and revealing the AFL's efforts at manipulation.

The book is well edited, with only ten typographical errors, a useful list of acronyms, and a good index. Aside from Lademacher's essay, the German is easy reading, but, since many quotations are in original languages, one must read both French and Italian to enjoy the full flavor of the evidence.

ROBERT W. SELLEN
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J. F. C. HARRISON. *The English Common People: A Social History from the Norman Conquest to the Present*. To-

towa, N.J.: Barnes and Noble or Croom Helm, London. 1984. Pp. 445. \$26.50.

English historians discovered the common man over a century ago, and his story has been written and rewritten ever since. Thus, it is a surprise to learn from J. F. C. Harrison that "the historian shuts himself off from ever finding the common people as they saw themselves and then proclaims that the common people have no history, or history worth taking seriously" (p. 17). We wonder who Harrison's hypothetical historian may be, for he cannot belong to the large group just cited by Harrison for recent work using novel sources and techniques, those who have lately studied crowds, customs, communities, and censuses. The outdated criticism is doubly unfortunate in that the story as told here does draw on new lines of social history to produce a lively and readable survey—one that would be more attractive without its defensive introduction.

The English common people begin their journey as the "English peasantry." Harrison justifies the term by pointing to the value of comparative study, but that is not his purpose here. More unsettling is his definition of "peasant," which seems to include all levels of free and dependent rural population. The last chapter of this section discusses the "growth of freedom" in the later Middle Ages. Certainly, the end of villeinage signaled the arrival of a different social order, and, just as surely, today's social historians will ascribe the change to more than a quest for "freedom," as epitomized in protests such as the Peasants' Revolt.

In the early modern period we begin to hear the authentic voices of the "labouring people." Harrison's second section outlines conditions between the sixteenth and eighteenth centuries amid the turmoil of reformation and revolution. He cites Peter Laslett's description of a "one-class" society but spends most of this section working with the famous outline of English society produced by Gregory King in 1688—which picture was anything but a "one-class" society.

Harrison then moves into the nineteenth century (1780–1880) and discusses the arrival of the "working class." As befits his own expertise, this is Harrison's longest section, but it also has more detail because this age was critical in the development of self-conscious popular political expression. The main theme of the section is the impact of the Industrial Revolution, which is handled with balance and skill. The last section, "The People, 1880–1980," is the shortest but the most problematic. Harrison notes correctly that until recently this period has been described almost solely in terms of "labour history," although trade unionists were not a majority of the people or the working class. Here the "labour version" is condensed into a single

chapter, and it is joined by two more general chapters on the effects of war, the dole, and postwar social reform.

The English Common People is a readable survey with a sensitive populist approach. Except for some inordinately long extracts, Harrison's writing is fluid and clear. His approach to social history plays down political events while it plays up popular political development ("Growth of Freedom" in chap. 3; "Protest and Revolt" in chap. 8). This is in contrast to recent works of some other social historians (Asa Briggs, Roy Porter, Keith Wrightson) who describe the society as a whole and include its political structure and behavior. This coincidence could provide an interesting test of which version of their own history "ordinary" readers will prefer.

RICHARD S. TOMPSON
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J. A. SHARPE. *Crime in Seventeenth-Century England: A County Study*. (Past and Present Publications.) New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1983. Pp. vii, 289. \$49.50.

This is the second full-length study of crime in the county of Essex to appear in the last decade. It is also the first devoted exclusively to the seventeenth century. As such, J. A. Sharpe's sinewy monograph (211 pages of text, including 37 maps, figures, and tables) bridges the chronological gap between Joel Samaha's study of Elizabethan Essex and the more substantial, but as yet unpublished, works of John Beattie on eighteenth-century Surrey and Sussex and of Douglas Hay on Staffordshire in the second half of that century.

All four studies take as their common base the systematic investigation of criminal court records—a methodology now firmly rehabilitated after earlier criticisms. The core of Sharpe's book is an analysis of the 8,557 offences indicted at Essex assizes and quarter sessions and in the court of King's Bench between 1620 and 1680. Excursions into the leet and archdeacons's courts and, less comfortably, into the records of the northern assize circuit add color to this impressive statistical base. Throughout, Sharpe is mindful of the limitations of judicial records, the ever-present "dark figure" of unrecorded criminality, and the possible impact of undiscerned shifts in prosecutorial energy; thus, he avoids the sociological fancies that disfigured Samaha's earlier study. His only serious lapse is to sniff at a red herring trailed by Alan Macfarlane in his enigmatic *The Justice and the Mare's Ale* (1981) and pounced on by Lawrence Stone in a recent *Past & Present* article—that there were long-term trends in crimes of violence. Sharpe's conclusions—that all

social groups were quick to anger and ready to use violence and that English society in the seventeenth century was more violent than its modern-day counterpart—sit uncomfortably on evidence that seems, at best, ambiguous.

Generally, however, historians working in this field will welcome Sharpe's cautious reservations and his repeated reminder that, to be meaningful, criminal statistics must be seen in their local context. Historians will also be struck by the familiarity of that theme and, indeed, by the surprisingly dated quality of most of Sharpe's findings. In the decade since the basic research for this book was completed, the study of historical crime has advanced with spectacular rapidity. Pleas for village studies no longer seem very remarkable, nor does the suggestion that most seventeenth-century thefts were opportunistic and most violence spontaneous. In the last few years scholarly attention has shifted decisively away from the study of crimes and criminals to the more subtle interactions between criminal law and those responsible for its execution: jury composition and verdicts, the sources and intensity of prosecutorial energy, mitigatory factors, and judicial discretion generally. It is, as always, niggardly to criticize an author for having written the wrong book. Ten years ago this would have been a pioneering study. That it now seems simply a good, slightly old-fashioned introduction to the study of early modern delinquency is testimony to the remarkable vitality of the history of crime.

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J. T. CLIFFE. *The Puritan Gentry: The Great Puritan Families of Early Stuart England*. Boston: Routledge and Kegan Paul. 1984. Pp. v, 313. \$25.00.

Determining the relative importance of religion in English life during the seventeenth century remains a challenging endeavor for historical study. J. T. Cliffe's new contribution dealing with the Puritan gentry of early Stuart England is a valuable addition to an ever-growing literature. The author has sifted through the usual primary and secondary sources and has also examined the private papers of numerous gentry families (123 to be exact). The result is a pleasantly written, largely anecdotal account of the attitudes and life-styles of this important segment of England's ruling class. Suitable for general readers, it contains important materials for the specialist as well.

The Puritan Gentry presents a definite point of view regarding the role of seventeenth-century religion, asserting from the very outset that religious beliefs cannot be seen merely as a reflection of economic and political interests. This was a profoundly spirit-

ual age, and these people, holding devout beliefs, followed the dictates of their religion in their behavior patterns. Defining the Puritans as "fundamentalists *par excellence*," Cliffe describes them as a special elect, "a small minority," set apart from the rest of society. Yet one of the ironies of this book is that it provides proof for the opposite conclusion, namely, that the Puritans differed only slightly from their fellow gentry in the manner in which they conducted their lives. Moreover, they never challenged the character of English society, accepting as given the supremacy of the wealthy landed classes. This is a fundamental point and in opposition to historians such as Lawrence Stone who see Puritanism as a corrosive of the social hierarchy and to those like Christopher Hill who describe a whole different way of life for the Puritans.

Even during the reign of Charles I, this Puritan gentry did very well financially, and, just like their Anglican brethren, they "accepted that riches were held in trust from God and carried with them social responsibilities which were divinely ordained." Puritan gentry rushed to accept the "inflation of honors," the baronetcies, and knighthoods. They not only intermarried with fellow Puritans but also married Anglicans and, on occasion, even Roman Catholics. There are examples of Puritans behaving immorally, gambling, and participating in the decadent aristocratic sport of horse racing.

Another interesting conclusion presented is that most of the Puritan gentry did not suffer substantially from Laudian policies. As the wealthy and powerful of the land, holding key local positions like justice of the peace, they were able to soften the impact of "Thorough." For the most part, they retained control over patronage, lay and ecclesiastic, so that their own religious needs remained satisfied throughout the era.

Cliffe ends his account of these Puritan families with a description of the religious changes, such as they were, during the Short Parliament and the first year of the Long Parliament. No explanation is given for why this section is appended, nor for the sudden termination of the book in 1641. This is a pity because the author appears to be somewhat puzzled by the opposition's reluctance to resolve the major issue of church government. Oddly enough, this book indirectly provides a partial answer to the puzzle. Not having been terribly inconvenienced by Anglican policies and unwilling to take measures that might have threatened the nature of English society, the Puritans chose to go slowly on the religious issue. In addition, shrewd parliamentary leaders like John Pym wisely shelved the matter of church settlement to avoid needless distractions from the urgent consideration of handling Charles I, especially when unity remained a priority. Regarding the outset of the English Revolution, Oliver

Cromwell once observed, "Religion was not the thing at first contested for." Perhaps it is time that English historians took the protector's words seriously.

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ROBIN CLIFTON. *The Last Popular Rebellion: The Western Rising of 1685*. New York: St. Martin's or Maurice Temple Smith, London. 1984. Pp. 308. £17.50.

In that sad summer of 1685, the duke of Monmouth, Charles II's illegitimate son, chose to challenge his uncle, James II, in a desperate gamble for the throne of England. The scene of his rebellion, the southwestern shires, has since lured more than a dozen authors to explore the rebellion's setting, its prospects, and its ultimate and seemingly inevitable failure.

The latest and possibly last analyst is Robin Clifton. His intelligent, albeit predictable, approach divides the topic into a colorful portrait of the Western Country and its people; Monmouth's drift into the politics of exclusion (Clifton believes Monmouth was drawn by honor finally to resist James II); the rebellion itself, which is handsomely covered in some of the best battle narrative of the period; and his conclusions regarding the participants and their cause.

But a mystery of sorts ensues on reading Clifton's splendid reconstruction: why did he write it? Peter Earle covered precisely the same ground in somewhat the same fashion seven years earlier in *Monmouth's Rebels*. With some major differences—Earle, for example, neglected Monmouth's leadership in his study—Clifton's work shows the same sensitive handling of issues and events as Earle's study. Both Earle and Clifton made much of the British Library's Add. MSS. 30077, the so-called Constables' List, or what Earle calls the Monmouth Roll. The list contains the names of many who were reported to the grand jury as possible rebels to be tried under the "bloody assizes." Oddly, Earle claimed the list contains the names of 2,611 rebels; Clifton says 2,691. Although both agree that a study of the list indicates that Monmouth's army was constituted, not of mean, lowly, peasant rabble, as Thomas Babington Macaulay and David Ogg asserted, but of sturdy craftsmen, responsible farmers, and local leaders. Their separate computer breakdowns, however, reconstitute the rebel army into different social and economic categories. The mystery, of course, is that Clifton writes not a word of his predecessor's parallel study done seven years before.

A reviewer thus confronted with such an anomaly is expected to judge which of the two works, seemingly to have been written without the authors' knowledge of each other and yet obviously the works of two independent and creative intellects, is the superior. After considerable scrutiny, the judgment is that they are superb supplements to each other, complementing each other's lapses; they should have been made a joint venture. One can only hope that, if it has not already happened, this review can bring the two authors together. In any case, the work of Clifton and Earle should bring the pursuit of the western rebels to an academic end.

GERALD M. STRAKA
University of Delaware

J. A. W. GUNN. *Beyond Liberty and Property: The Process of Self-Recognition in Eighteenth-Century Political Thought*. (McGill-Queen's Studies in the History of Ideas, number 6.) Kingston, Canada: McGill-Queen's University Press. 1983. Pp. 331. \$35.00.

J. A. W. Gunn's seven essays explore some infrequently traveled roads in the minds of eighteenth-century politicians and pamphleteers. He charts new territory and puts familiar subjects in perspectives different from those in which they usually appear. His principal theme, a significant contribution to our understanding of the changes in and increasing sophistication of popular political thought, is how the men of the eighteenth century came to terms with the changing political reality around them—how they fitted explanations of new forces (such as public opinion and extraparliamentary pressure groups of various kinds) into the increasingly outdated framework of platitudes and categories received from the later seventeenth century.

In his search for the ways in which rhetoric was adjusted to suit the facts, Gunn concentrates on the notions expressed by commonplace political pamphleteers and orators rather than on the ideas of the century's giants of political thought. The thoughts of Lord Bolingbroke, David Hume, Edmund Burke, and even John Locke appear rarely and only as distant background. By concentrating on ordinary notions in average minds, Gunn and other recent scholars, including Reed Browning and John Brewer, are coming steadily closer to recovering the confused ideas in the minds of most contemporary politicians. The rediscovery of this intellectual world is helping to bring back ideas—banished by Sir Lewis Namier and his followers—as one of the formative forces in eighteenth-century politics.

Some of the movements of political ideas that Gunn reveals are surprising, especially in the long (and most original) essay on the hardy survival of

High-Tory thought throughout this very Whig century. Views that had flourished in the reign of Ann, indeed even before the revolution, were revived vigorously in the 1760s and became totally respectable again by the 1790s. Was there, perhaps, a new Toryism in the reign of George III after all? Two other essays are concerned with the ideological acceptance of "public opinion" and extraparliamentary pressure groups as these forces became more prominent from the middle of the century. These intruders in political life were first treated as would-be fourth estates trying to usurp the prerogatives of the original three—King, Lords, and Commons—but were gradually accepted as fundamental parts of the constitution. By the early nineteenth century, they were, in fact, often seen as preserving that very balance they had originally been accused of over-setting. A fourth essay discusses, rather too briefly, the changing definition of "liberty" from the negative context of freedom to act without governmental coercion to the positive right to participate actively in government.

The remaining three essays are not quite as general in implication as the preceding four, but they are by no means mere footnotes to the history of Georgian political ideas. The first traces a prolonged debate about the danger of "legal tyranny," by which the forms of the "free constitution" that England enjoyed might be perverted into practical tyranny by ministerial corruption. This discussion is rich in examples of the strained classical analogies that so often isolated polemical debate from reality earlier in the eighteenth century. Another essay discusses Bernard Mandeville's development of "private vice, public benefit" as Whig propaganda, ridiculing patriot arguments for "virtuous poverty" as the ideal polity for a free society. The third moves to the other end of the century and the ideas of David Williams, a prolific pamphleteer of some originality, who began as a commonplace radical propagandist in the 1770s but gradually developed an increasingly sophisticated concept of political organicism, prefiguring nineteenth-century ideas. His intellectual evolution is not a particularly exciting story, but one that underlines how old-fashioned radical rhetoric, like the more conservative rhetoric that is the subject of the other essays, changed as eighteenth-century pamphleteers became more critically aware of the actual state of their institutions—how the "process of self-recognition" was at work everywhere in the world of political ideology. Taken all together, Gunn's studies add substantially to the steadily increasing richness and complexity of our knowledge of eighteenth-century political thought and of the ideological underpinnings of the age.

ROBERT A. SMITH
Emory University

BREAN S. HAMMOND. *Pope and Bolingbroke: A Study of Friendship and Influence*. Columbia: University of Missouri Press. 1984. Pp. 190. \$25.00.

This fine monograph focuses on a pertinent yet relatively neglected topic, namely, the relations between Alexander Pope and Lord Bolingbroke. Brean S. Hammond competently demonstrates the many affinities between these two writers and explains the significance of their friendship to the cultural life of early eighteenth-century England. The author develops the thesis that Pope's satire and Bolingbroke's political philosophy were two facets of a campaign to thwart the demise of England's moral fiber. This interdisciplinary study contains nine lengthy and detailed chapters and is set within a chronological and topical framework. Through effective literary analysis and biographical profiles, Hammond offers novel and exciting interpretations about the thinking and behavior of Pope and Bolingbroke.

The author carefully chronicles the evolution of the Bolingbroke and Pope friendship and sheds much light on literary and political ideas advanced in some of their major writings. Hammond believes that Pope and Bolingbroke probably met before 1715, prior to Bolingbroke's exile to France, and shows that both writers, after Bolingbroke's return to England in the early 1720s, lived near each other and developed a "freemasonry of two." Their intimate association is attributed to Pope's sensitivity about being Catholic, his willingness to be subordinate to Bolingbroke, and the mutual interests of both writers in literature, philosophy, and politics. Chapters in the first half of the book vividly portray the cultural activities at Pope's villa at Twickenham and those at Bolingbroke's estate at Dawley Farm. These chapters also describe the emergence at Dawley of a political circle in opposition to the policies of Walpole, examine major ideas appearing in the articles of the *Craftsman*, and contain analyses of select poems and epistles of Pope that reveal his anti-Whig sentiments. Hammond, too, devotes considerable attention to the importance of the "Essay on Man" and to the development of Pope's political ideologies. After assessing the arguments of many eminent literary critics about the chronology, model, and substance of the essay, he argues that this work reflects the influence of Bolingbroke's views concerning society and the state. In the final chapters of the book, Hammond examines Pope's other works to show that he embraced the cause of the patriots and, like his friend Bolingbroke, advocated in opposition to Whiggism a moral philosophy to reform English political and economic institutions.

This work is an impressive and a substantive study. It is based on a meticulous examination of

both primary sources and secondary studies and contains persuasive interpretations regarding the intimate partnership of Pope and Bolingbroke. In convincing fashion, Hammond evaluates the importance of this friendship within the context of the Enlightenment in early Georgian England. The book contains a splendid introductory chapter devoted to a review of the historiography regarding Pope and Bolingbroke. The book, however, regrettably lacks a conclusion and contains several mistakes in pagination. Yet these are minor criticisms of a work that assuredly will be recognized as a definitive study in the field.

R. WILLIAM WEISBERGER
Butler County Community College

DOREEN M. ROSMAN. *Evangelicals and Culture*. London: Croom Helm, distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. 262. \$29.00.

The idea that English Evangelicals were hostile to art, imagination, and the use of the intellect outside of a narrowly religious sphere—in short, to culture in its wider sense—is an old one. When Matthew Arnold pilloried the adherents of Evangelicalism's nonconformist wing as "Philistines," he was in a tradition that went back through the sons of Evangelicals to the Evangelicals themselves. Doreen M. Rosman's book investigates how accurate and how fair such a picture is for the first generations of the movement, up to 1830.

She has little difficulty showing that, for many Anglican Evangelicals and their dissenting and Methodist brethren, it is not fair at all. They did not allow "serious" religion to keep them from an educated and frequently wide interest in art, literature, and learning in general. They had their blind spots, of course, but these were not notably wider than those of other educated people of their time. She insists convincingly that Evangelical thinkers, despite their revolt against the world and the devil, were very much children of their own time.

Despite this welcome redressing of the balance, she insists that, at bottom, the Evangelicals were unable to accept culture in any full and proper way and that this inability was rooted in their theology. Most recent work on the Evangelicals has adopted what may be called a humanist point of view, either damning them because they were obscurantist and devious or praising them because they did good to mankind in this world. Rosman shares their interest in religion but sees their theology as weak; they separated the spiritual from the worldly to such an extent that they could not see that God's creation was good. Therefore, they could not see that the exercise of mankind's powers of creation and imagination—culture, in short—was not only good but

godly. She is clearly a disciple of that great theologian (or "muddy mystic") Frederick Denison Maurice. Not all readers will share that enthusiasm. But it is refreshing and critically fair to assess the Evangelicals in categories they would have understood, even if they would have objected strongly to her verdict.

JAMES NICOLL COOPER
Carleton University

JOHN M. MACKENZIE. *Propaganda and Empire: The Manipulation of British Public Opinion, 1810-1960*. Dover, N. H.: Manchester University Press. 1984. Pp. 277. \$35.00.

By analyzing how new developments in mass communications were used to "manipulate" British public opinion, John M. MacKenzie shows that imperialism as a cultural phenomenon was not a casualty of World War I but has flourished down to the present; the Falklands War, not the year 1960, is perhaps his real terminus. Increasing numbers of intellectuals opposed imperialism after 1914, but radio and cinema took over where music hall and jingoist press left off. After World War II, the author argues, "popular imperialism" grew "nostalgic," as if acknowledging its distance from political reality. But MacKenzie shows that imperialism was a powerful factor in public opinion through the interwar years.

MacKenzie also shows that imperialism was promoted by a wide variety of media. He extends the range of sources of pro-imperialist sentiment described by J. A. Hobson in *The Psychology of Jingoism* (1901) and adds to Daniel Headrick's analysis of technology in *The Tools of Empire* (1981). MacKenzie's chapters on imperialist organizations, exhibitions, textbooks, radio broadcasts, cinema, and mass culture "ephemera" such as picture postcards are especially original. Successive governments hardly needed to generate imperialist propaganda, so eager were voluntary and commercial enterprises to do so. Children's literature offers a paradigm: worried about the nefarious influence of "penny dreadfuls," various mid-Victorian publicists discovered in imperial adventure stories a way to present "safe," conservative values in exciting packages. The same combination of safe values with adventure guaranteed the continued popularity of imperialism in other media, including cinema.

But did such sources of popularity involve "manipulating" public opinion as imperialist organizations sought to do? "Propaganda," "manipulation," and "public opinion" are all slippery terms. Perhaps MacKenzie should be congratulated for sticking to those terms rather than insisting on more elusive ones like "ideology" and "cultural hegemony"—

though he does refer to imperialism as a "core ideology." But it is not always clear what impact he thinks a given cultural phenomenon had on "public opinion," except that it had an impact.

Also, although MacKenzie demonstrates the continued life of "popular imperialism" after 1914, why not before 1880? He might have pushed his investigation backward to the propagandizing of the East India Company, the Royal Geographical Society, the abolitionist and missionary movements, and the emigration and colonization societies of the early 1800s. Perhaps these were not "imperialist" in the same way as the Primrose League, but Ronald Robinson, John Gallagher, and others have demolished the myth that imperialist expansion was a new factor in politics in the 1880s. Why then should imperialism be new to British culture after 1880? Only the extreme emphasis given to imperialism by the mass media after 1880 seems really new.

But MacKenzie can be thanked for providing a careful, wide-ranging account of the continued life of imperialism in British culture down to 1960 and beyond. He is surely right that "the Falklands war of 1982 aroused many echoes of . . . popular imperialism" (p. 11). He demonstrates that, despite an actual decline and fall, in terms of public opinion the sun has still not set on the British empire.

PATRICK BRANTLINGER
Indiana University

DOROTHY THOMPSON. *The Chartist: Popular Politics in the Industrial Revolution*. New York: Pantheon. 1984. Pp. 399. \$12.95.

Chartist scholars must now adjust their bookshelves to accommodate the penultimate study of Chartism. This survey replaces those of R. G. Gammage, Mark Hovell, Julius West, and J. T. Ward because of its rich synthesis and exceeds David Jones's contribution in its analytical acumen. Dorothy Thompson's book contains a refreshing narrative account and an analytical social history free from jargon, impenetrable statistical analysis, and ideological obscurantism. But Thompson's ultimate contribution to Chartist historiography is her persistent reminder that the Chartist community consisted of British men and women—perhaps even youth—who sought control over their lives and pursued political redress for their grievances. She demonstrates the Chartists' depth of belief in the possibility of achieving political changes via the Six Points of the People's Charter to improve their living circumstances, and she delineates the movement's social appeal. In parts 1 and 3 she conducts her readers through a whirlwind tour of Chartism, high and low, far and wide, throughout Britain and, at times, beyond. The fast-paced

narrative is punctuated in part 2 by an analysis of the social composition of Chartist community.

The world of Thompson's Chartism is more complicated than is usually described. The participants' options exceeded the dialectic opposites of moral or physical force, independent action or middle-class alliance, London's interests or those of the provincial hinterland, backward perspectives or modernizing thrusts; instead, Thompson tells the reader, "If we are to listen sympathetically to the voice of Chartism, we should perhaps set aside some preconceptions about historical absolutes and listen to the contemporary debate without being too fixated on our *ex post facto* knowledge of its outcome" (pp. 4-5). To the Chartists the petition and the pressure tactics of mass meetings and rhetoric offered real possibilities for affecting change. Obtaining suffrage was central to their hopes. The desire for political control over their economic life harbored an intent to slow changes in technology and in patterns of work. This political motive, Thompson argues, represented neither old paternalism nor an attack on existing property relations but injected into the political arena an unacceptable concept of labor as working-class property just as deserving of political protection as any other property. Yet this thrust, Thompson intimates, emanated from the movement's community rather than from its productive experience. She locates the special expression of this community life in the manufacturing districts where, under the strains of industrialization, a desire existed to preserve traditional ways of work and living. Thompson concludes that the perspectives from these variegated local communities had a dichotomous effect on the national Chartist movement, both fracturing and cementing it.

Who, then, participated in this struggle? Thompson deserves praise for her herculean effort at rescuing the women of the Chartist movement from obscurity and for her inclusion of brief biographical sketches of prominent local and national Chartist leaders. Her analysis of the social composition of Chartism is unparalleled and establishes the participation of both skilled and unskilled workers. A determination of the full range of Chartist participants is impossible, according to Thompson, because the evidence regarding participation is distorted by the degree of independence that members of various occupations enjoyed from employer victimization. Thus, women and agricultural laborers are probably underrepresented among known Chartists, whereas shoemakers and tailors, who enjoyed considerable independence, are probably overrepresented. Smiths, metalworkers, wood workers, carpenters, building tradesmen, the various textile workers, and others are found to have participated in and provided leadership for Chartism roughly in

proportion to the national employment distribution of each.

Thompson has written a sympathetic yet learned account of the political and social expression of Chartist fears and reform aspirations. The study is the product of mature scholarship. Where limited evidence dictates or additional research is required, her commentary is circumspect. This is a great social history that ought to be read as a companion volume to Edward Thompson's *The Making of the English Working Class*.

DUANE C. ANDERSON
El Reno Junior College

ANTHONY S. WOHL. *Endangered Lives: Public Health in Victorian Britain*. Cambridge: Harvard University Press. 1983. Pp. 440. \$20.00.

Anthony S. Wohl, in his introduction to *Endangered Lives*, writes: "Countless articles and books have been written on the Victorian mind, but relatively few on the Victorian body. There should surely be no excuse needed for a book which examines the connection between physical and social environment and the human body in Victorian Britain . . . in this book the emphasis is less upon central and local reform agencies, public administration, and medical and scientific aspects than upon the social and physical environment and its effects upon the public's health" (pp. 2-3). This is confusing because Wohl has produced a well-organized, frequently insightful, old-fashioned history of public health, in which the emphasis is on central and local administration, the Victorian mind (in its application to public health), and the physical environment.

The continuing themes in the work are the impact of very rapid urbanization on health, the lot of the working-class family (because Wohl rightly regards poverty as engendering and perpetuating all other problems), and the public health movement as a Victorian moral crusade. These are skillfully woven into successive chapters on infant mortality, diet, scavenging and sewerage, epidemic diseases, central administration of state medicine, local administration of public health, atmospheric and river pollution, industrial diseases, overcrowding, and concern for racial degeneration. His insight into the psychology of professional administrators in steering the elected authorities to correct the overt abuses rather than the root causes that would have required state intervention is particularly acute. So, too, is his understanding of the ways in which local governments were gradually converted to the need for widening their responsibility for public health and of the retarding effect that the cumulative nature of pollution placed (and still places) on corrective measures.

The weakest parts of the work have to do with the medical core of public health ("the Victorian body") and the social and economic environment. The chapters on epidemic and industrial diseases show rather limited understanding of the etiology of the diseases and, hence, of their connection to the environment. Did major epidemics decline in the eighteenth century as a result of improved food supplies (p. 118)? Could sewerage have reduced the *virulence* of typhoid fever (p. 128)? Surely anthrax was enzootic, not endemic (p. 265). Was phossy jaw, which Wohl discusses at length, a more serious health hazard than industrially caused cancers or phthisis, which he hardly mentions? What economic changes made possible the substantial improvement in working-class diet from the 1870s on? How did it become possible, quite suddenly toward the end of the century, for local authorities to borrow on a large scale for public health improvements? With such omissions supplied, *Endangered Lives* might have been more than the useful, well-documented history of public health that it is and that Wohl apparently set himself to replace.

JOHN NORRIS
University of British Columbia

MARK GIROUARD. *Victorian Pubs*. New Haven: Yale University Press. 1984. Pp. 260. Cloth \$35.00, paper \$16.95.

If poetry is best sipped like wine, history, according to A. J. P. Taylor, should be taken in long draughts like beer. What, then, could be more agreeably gulpworthy than a paperback reissue of Mark Girouard's masterly treatment of the Victorian pub? The man who has so successfully guided us through the halls and kitchens of the country house here swaggers round the promontory bars and select snugs of the people's palaces. With lavish illustrations, Girouard details the interiors, explains the changes in logistics and architectural style, and sets the pub within the broader context of social and political history.

Concentrating on London, the book traces the pub's development from the traditional tavern through the gin palace to its most seductive efflorescence in the great boom of the 1890s as the trade sought to adapt to the new demands of city life, fight off temperance opposition, and make its fortune. Girouard is particularly good on the genial rapacity of the publican-speculators; he also treats with architects, brewers, contractors, barmaids (including snapshots of some lumpish specimens), drinkers, and drink. The whole is accomplished with wit and erudition, enough to make the more pedestrian academic choke on his stout with envy. And, though the author is plainly and correctly in love with his

subject, unlike the Victorian tippler he stops short of sentimentality and excess.

If this is a classic by learned as well as popular standards, it should not close off the subject to others. The dialectic between drink and temperance touched on here and ably unraveled by Brian Harrison to 1872 needs further attention. This could illuminate the politics of the later period, particularly at the local level where the publican's role as vestryman and councillor is underresearched, as well as explicate the cultural dynamics of prohibition and permissiveness that gave public life in Britain its distinctive ambiguity. Girouard goes beyond the nostalgic impressionism of other pub books to get at the specific social genius of the institution, but it would be good to see this set within a comparative study of other sociable assemblies. Could historians begin to look at music halls, restaurants, hotels, and exhibitions with the same skills they have focused on asylums, prisons, and poor-houses? The balance of closed and negotiable space and codes of management in these places also articulated ideologies and cloaked hegemony in a culture universally sensitive to the process of selection and exclusivity.

PETER BAILEY
University of Manitoba

STEVEN TISCHLER. *Footballers and Businessmen: The Origins of Professional Soccer in England*. New York: Holmes and Meier. 1981. Pp. x, 154. \$27.50.

This book seeks to relate the development of football in the four decades or so before 1914 to wider societal movements, particularly structural changes in industry and associated class relations. Steven Tischler argues that professional football was a business like any other and that the values of the marketplace dominated the actions of those middle-class men who ran the clubs. Such attitudes produced the inevitable collective response from the worker-players, culminating in the conflict of 1909 that nearly produced a strike.

But was football just another capitalist industry? Direct financial benefits to shareholders were limited and often nonexistent. Indirect financial rewards may have accrued to a few directors, and psychological benefits to others, but directors were not acting as profit maximizers. Competition in sport, unlike in business, has to be controlled. The opponent is there to be beaten but needs to be left alive so that he can return next year.

Were the players like other workers? They were certainly better paid, and it is difficult to believe that, since training was rudimentary, they exercised no control over how they did their jobs. In general, there is little in this book about the life-style of the

players and how it resembled or differed from that of other workmen. In 1909 the players wanted an end to the maximum wage and a return to the free market. Were other workmen trying to enforce such laissez-faire notions?

Tischler's Marxism is too mechanistic. He exhibits none of the subtleties of the hegemonists. Given the nature of his argument, a more systematic attempt should have been made to examine the finances of less successful clubs. He is silent about how playing grounds were acquired and developed. His research is painstaking but limited. Professional sport has similarities with capitalist industry, but there are differences, too, differences that Tischler consistently underrates.

TONY MASON
University of Warwick

A. W. BRIAN SIMPSON. *Cannibalism and the Common Law: The Story of the Tragic Last Voyage of the Mignonette and the Strange Legal Proceedings to Which It Gave Rise*. Chicago: University of Chicago Press. 1984. Pp. xiv, 353. \$25.00.

A. W. Brian Simpson has published basic treatises on land law, contracts, and other concepts of common law. Here he has stumbled on a different approach. This book, a comparative study of cannibalism as a custom of the sea and as a rationale for murder, treats the legal question of the defense of necessity in common law. The occasion is the centennial of *Regina v. Dudley and Stephens*, a famous "leading case."

In July 1884 the fifty-foot yacht, *Mignonette*, en route to its new owner in Australia with a contracted crew of four, foundered in a storm in the South Atlantic. The crew barely saved themselves in a dinghy and drifted for twenty-four days before being rescued and returned to England. Three crewmen survived: the captain, Tom Dudley; the mate, Edwin Stephens; and a seaman, Edmund Brooks, who became the essential prosecution witness. The youngest crewman, Richard Parker—age seventeen, illiterate, and allegedly on the verge of death—was killed and eaten four days before the rescue. In such circumstances maritime custom sanctioned the casting of lots, an action discussed but not implemented in this instance.

Dudley and Stephens were tried for murder before the Exeter assizes, but the case was adjourned because of a "special verdict" (the jury was persuaded not to decide on guilt or innocence). The case was resumed in London before a panel of judges called Queen's Bench Division. The judges found the two guilty and handed down sentences of death, which royal prerogative commuted to imprisonment for six months.

For Simpson, the case is the central authority for the defense of necessity. The facts "raise in dramatic form the problem of reconciling the instinct for survival with a moral code that respects the sanctity of human life" (p. x). He claims that the details and background have been neglected and that only summarized law reports and sensationalized mythical accounts have survived. But Michael Mallin in the *University of Chicago Law Review* (1967) cites the case as remarkable because of the "special verdict," a rarely used procedural device.

Simpson aspires toward the best of two worlds. Like Mallin, Simpson could have expounded his thesis within the limited confines of a scholarly article. He has exhaustively researched all types of sources: interviews with experts and survivors, archives, the public record, manuscript collections, literary works, newspapers, and folklore. His command and use of ballads and rhymes of the sea associated with shipwreck and cannibalism are unsurpassed. But Simpson also wanted to write a popular account with high public appeal and no formidable scholarly apparatus. The result is numbing boredom and repetitious padding—page after page of accounts (at least two dozen) of disasters and cannibalism on land and sea, foreign and domestic, past and present, and inside and outside the "common law world" (for example, in Norway). He denigrates sensationalized accounts, ghoulishness, and mythologizing (as, for example, in Donald McCormick's *Blood on the Sea* [1962]), yet he himself exploits trivia, superstition, and puns. There are repeated instances where the use of footnotes is demanded; there are inconsistencies, errors, and misspellings. Thus, he fails on both counts. Overall, the book is a disappointment.

EUGENE L. RASOR
Emory and Henry College

JAMES GOLDRICK. *The King's Ships Were at Sea: The War in the North Sea, August 1914–February 1915*. Annapolis, Md.: Naval Institute. 1984. Pp. xv, 356. \$21.95.

Is this book really necessary? The question is perhaps unfair or even meaningless (how many books are?), but it is a natural one in view of the number of well-crafted works traversing the same waters and one that is raised by Lieutenant (in the Royal Australian Navy) James Goldrick himself in the preface. The title itself is something of a warning; it is an unacknowledged quote from Churchill's *World Crisis*.

And yet the subtitle strikes just the right note. The intent is clear: "a comprehensive history of operations" (p. ix), viewed as a causal continuum, before the opening of the Dardanelles expedition, which

was envisaged by the war leadership as a historic opportunity for the application of British seapower outside home waters. Goldrick's book, apparently the outgrowth of a study tour in England, makes good use of naval staff monographs, Admiralty files in the Public Record Office, and a wide variety of personal manuscripts of naval officers. While the focus is on the British, the German side is not neglected. The writing is clear, equable, and well directed, less obscure than Julian Corbett's if not as sparkling as Arthur Marder's. The Naval Institute Press has done its part by producing a volume attractive to the eye.

The war in the North Sea during these opening months consisted on both sides mainly of feints, probes, and parries difficult to summarize. Goldrick stresses the inflexibility and narrowness of vision of the senior professional leadership: "he [First Sea Lord Prince Louis of Battenberg] simply could not keep an adequate mental picture of events" (p. 154). The pervasiveness of failures in communication, the outgrowth of a tendency both at the Admiralty and at operational command levels to keep one's own counsel, is once again underlined. More originally, Goldrick notes both the degree to which technological developments (mines, submarines, aircraft, wireless) had already rendered capital ships ineffective and the inability of naval leadership to adjust adequately to these changes with other than blank denial or an overly defensive posture. The fact remained "that battleships could not be sent into enemy waters without the risk of considerable losses" (p. 237). As at the Western Front, strategic deadlock had set in. As at the Western Front, it was not at all clear how to remove it.

Goldrick concludes with some apt parallels and warnings for the present. He has written an enjoyable and stimulating work.

PAUL GUINN
State University of New York,
Buffalo

STEPHEN KOSS. *The Rise and Fall of the Political Press in Britain*. Volume 2, *The Twentieth Century*. Chapel Hill: University of North Carolina Press. 1984. Pp. x, 718. \$34.00.

Praise and admiration for Stephen Koss's volume 1 on the nineteenth century (1981) will be more than matched by that accorded to volume 2. And again one must single out for special emphasis the author's massive research in "manuscript sources," many of which were hitherto unused, even unknown. But this is matched by the store of knowledge that Koss brings to this task and the clarity and wit with which he tells his story.

He sets the stage with his general remark that "the political press must be seen as a contrivance on the part of diverse figures, all eager to bring pressure to bear on affairs of state" (p. 7). As the nineteenth century came to an end, the political press appeared to have had its day. Instead, with the revival of liberalism, the rise of the Labour party, the parliamentary crisis of 1909-11, and the advent of World War I, the scope of the political press was greatly extended. Half of this volume deals with those years. During the 1920s the political press is clearly in decline, a development that Koss follows until mid-century but does not always explain. Lord Beaverbrook's view "that the normal attitude of the Press towards the politicians must be one of complete independence" (p. 386) was widely held if indifferently followed.

One measure of Koss's contribution to knowledge and understanding can be found in areas where the press has already been well served: Koss's own study, *A. G. Gardiner and the "Daily News"* (1973), A. M. Gollin's *"The Observer" and J. L. Garvin, 1908-1914* (1960), this reviewer's *Radical Journalist: H. W. Massingham* (1974), David Ayerst's *The Manchester Guardian: Biography of a Newspaper* (1971), and R. Pound and G. Harmsworth's *Northcliffe* (1959).

An obvious comment and perhaps a criticism is that, with the exception of the *Manchester Guardian* and to a lesser extent the *Yorkshire Post*, Koss deals only with the London metropolitan press. "To all intents and purposes," he writes, "the London press was Britain's national press" (p. 2). This statement is subject to further inquiry. One also wishes for further comment on manuscript sources little known to other historians. But these reservations are pushed aside as we recognize that here is a fascinating book both for specialist and general reader alike.

ALFRED F. HAVIGHURST
Amherst College

HENRY PELLING. *The Labour Governments, 1945-51*. New York: St. Martin's. 1984. Pp. vii, 313. \$27.50.

This book is a critical survey of the two British governments presided over by Clement Attlee from 1945 to 1951. Within a compass of 300 pages the author deals with a wide range of topics including the elections of 1945 and 1950, the economic framework, the political background, and domestic, foreign, and imperial policy. Though not a definitive study, a task that Henry Pelling has deliberately eschewed because of the plethora of documentation, the work is based on a selective reading of the available primary material, both public and private. There is no bibliography, but this deficiency is partly remedied by the full chapter notes and the list of unpublished sources cited.

As might be expected from the doyen of British Labour historians, Pelling is particularly illuminating on the relations between party and government. His chapters on the nationalization projects and the Marshall Aid negotiations are also models of succinct analysis, skillful synthesis, and lucid exposition. Some sections, notably the one on foreign policy, suffer from the effort to compress and others from a strained attempt to achieve "balance." The introduction, which covers Labour history between 1919 and 1945 in fifteen pages, is patchy (and contains the assertion, not borne out by Ramsay MacDonald's latest biographer, that the 1931 proposal to reduce unemployment pay by 10 percent was rejected by a majority of the cabinet). But the important point is the extent to which the book succeeds in overcoming the obvious obstacles. This is not a point that Pelling cares to make in regard to his subject, but his book seems certain to become a standard text, much as Attlee's governments increasingly appear to be a point of reference for most sections of the British Labour movement.

And yet, taken as a whole, the book disappoints. It may be that, like certain Labour idealists in regard to their governments from 1945 to 1951, we have come to expect too much from the author. Previously, Pelling's talent for taking the passion (and rhetoric) out of Labour political history yielded valuable insights. Here, there are few surprises. We are left with a highly competent yet equally dry, almost dessicated, account of an extraordinary period. Pelling's emphasis is on the difficulties and weaknesses of the Labour governments. He concludes that there was no real planning of the economy, except in a negative sense (p. 256), that the creation of the welfare state was "due to the competitive nature of the electoral system" (p. 261), and that in the area of foreign policy the government was "even more at the mercy of the harsh nature of the post-war world than it was at home" (p. 266). Comparisons with Kenneth O. Morgan's longer and more widely documented *Labour in Power*, published a few months earlier, are inevitable because where Morgan is enthusiastic Pelling is gloomy. What may be said is that, together with Morgan, Pelling's book serves as an admirable introduction to postwar British history.

TREVOR BURRIDGE
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MICHAEL LYNCH. *Edinburgh and the Reformation*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1981. Pp. xv, 416. £18.00.

Michael Lynch has tapped Edinburgh's extensive records to produce an elaborate account of the city's politics during its greatest upheaval. A highly de-

tailed study—appendixes comprise nearly half of the book's more than four hundred pages—and, on the whole, a careful one, Lynch's Edinburgh emerges as a large town of perhaps thirteen thousand inhabitants that could rival Norwich as Britain's second city. Nevertheless, cramped in vision as well as space, its population was deeply conservative and inward looking. In contrast to the cities of Germany and Switzerland, its Reformation did not articulate class tensions or the interests of any particular social group. Also unlike these earlier Reformations, Edinburgh's did not result from a carefully engineered, internal revolution but from an external upheaval that only politicized the city's Protestants in 1559–60. The resulting Protestant regime came to power unexpectedly and held it uneasily throughout the 1560s. The new rulers had difficulty excluding Catholics from minor municipal offices and in instituting wide-ranging reform, and they were vulnerable to Mary's divisive tactics. Only after 1572 in the wake of the civil war did Protestantism rule securely, and, even so, another decade passed before it permeated and transformed Edinburgh society as a whole.

The truly striking feature of this Reformation is what a remarkably bloodless event it turned out to be. Despite the valiant efforts of recent scholars, Catholic martyrs remain exceedingly difficult to come by, and after 1560 martyrs of any kind are hard to locate. For all of the city's conservatism, we encounter very little ultra-Catholicism and not a whisper of the ideas and attitudes that would underwrite the Counter Reformation. The Catholic party in Edinburgh was fueled less by religious conviction than by resentment of the new regime. Many of its leaders appear never to have risked indulging in Catholic worship. None of its members ever sat on the city council after 1559. John Knox only shadowboxed with the Edinburgh Catholics; his real challenge was mobilizing the broad spectrum of Protestant opinion. Whatever the successes of Guisan militancy in France, neither the regent nor her daughter ever found ultra-Catholicism to be a viable policy in Scotland. Scotland would never experience anything like the French religious pogroms that culminated in the horrors of the St. Bartholomew Day Massacre.

Resistance to reform came from traditionalism rather than Catholicism. Burghal privilege, not religion, not ideology, normally moved the elites to action, and time and again this circumstance frustrated the radical activists. The course of the Reformation was charted by the city's "natural" rulers, and the interaction of "radicals" and "moderates" among them constituted a large part of Edinburgh's politics. Only at the end of the 1570s did an extra-institutional movement emerge as a powerful, autonomous force—in the process largely displacing

its conciliar leadership. Only in the wake of this development was a religious test imposed as a prerequisite of burghal status, directed not against the irrelevant Catholics but against radical Protestantism.

Protestantism in Edinburgh had more than one face, but what prompted men to adopt one rather than the other—or to adopt Protestantism at all? Lynch never really tells us. His study rather seeks to puncture the cult of Knox and to establish that the Protestant triumph was neither easy nor inevitable—surely to beat horses long since deceased. The Scottish Reformation did indeed produce men like John Erskine of Dun no less than Knox, and contemporary historians need to explore these options and what made them persuasive. Preoccupied with the illusion of historical “balance” and possessing an evident distaste for much of his subject, Lynch is ill-located to undertake this task. *Edinburgh and the Reformation* will undoubtedly prove a useful study but, like the old burgh, suffers from its traditionalism.

ARTHUR H. WILLIAMSON
New York University

THOMAS J. MORRISSEY. *Towards a National University: William Delany, S.J., 1835–1924; An Era of Initiative in Irish Education*. (Topics in Modern Irish History.) New Jersey: Humanities Press or Wolfhound Press, Dublin; distributed by Humanities Press, Atlantic Highlands, N.J. 1983. Pp. 425. \$26.00.

The figure of William Delany is familiar to readers of James Joyce's novel *Stephen Hero*. Delany, called Dr. Dillon in the novel, was described by Joyce as presenting “over the edge of the breviary a neat round head covered with curly gray hair and a very wrinkled face of an indescribable colour; the upper part was the colour of putty and the lower part was shot with slate colour.” As Thomas J. Morrissey shows in this study, Delany was something more than a footnote to a work of literature, although in the long run his fame may survive only because of that fact. During his long life (1835–1924) Delany was a man of considerable influence in Irish political and educational circles. He came to know Arthur (“Bloody”) Balfour and was invited to the viceregal lodge for dinner while Balfour was chief secretary. He was also a friend of John Morley and acted as a go-between when Morley's stepdaughter became a nun in Dublin. Morrissey's biography provides a sympathetic picture of a type of Irishman who was later to be stigmatized in “Irish-Ireland” circles as a “West Brit.”

Much of the interest of this book lies in the light that it sheds on the complexities that surrounded the problem of providing university education for

the Irish Catholic middle class, which rose to political power during the latter part of the nineteenth century and the early years of the twentieth. As Bishop Dwyer of Limerick put it: “The political centre of gravity in Ireland has shifted towards the Catholics. . . . In three provinces, practically all political and municipal power . . . have passed into their hands. It is a dangerous thing to leave them uneducated.” But where was an acceptable institution of higher education to be found? William E. Gladstone's plan in the early 1870s had been to establish a University of Ireland that would include a number of separate colleges, including Trinity College, Dublin. This project collapsed in the face of opposition from Trinity and the Catholic hierarchy, but the problem would not go away. When the compromise solution of an examining university was reached, the Royal University came into being but won little loyalty. It was to this body that University College, Dublin, located at 86 St. Stephens Green (formerly the buildings of Cardinal John Henry Newman's ill-fated Catholic University), was attached. Delany was its president and Joyce one of a number of able students.

The contrast between the wealth of Trinity and the ramshackle facilities offered by University College (which lacked even a decent library after the bishops moved its books to the Dublin seminary at Clonliffe) aroused a great deal of resentment, which eventually became a political issue. Morrissey shows how a major clash developed within Catholic circles about what should be regarded as the best solution. Archbishop Walsh of Dublin supported the old Gladstone scheme, which he looked on as a means of weakening a bastion of the ascendancy. Delany pressed for the establishment of a Catholic university on the lines once advocated for England by Henry E. Manning. A bitter conflict developed between the archbishop and the Jesuit, the outcome of which was, by and large, a victory for Delany. In 1908 the National University of Ireland was founded, which, though secular in principle, was Catholic in practice. Higher education in Ireland was now established de facto on sectarian lines: Church of Ireland at Trinity College, Presbyterians at Queen's Belfast, and Catholic at “the National.” In bringing about this solution, Delany had played a not inconsiderable role.

The book also has a good deal to say about Delany's relations with the student body of University College, Dublin, which though small in numbers (less than two hundred) made up for this with activism of various kinds. Cruise O'Brien, father of Irish historian Conor Cruise O'Brien, makes an appearance as a particularly sharp thorn in Delany's flesh, along with such figures as Tom Kettle and Francis Sheehy Skeffington. The highly emotive issue of compulsory Irish as an entrance require-

ment came up during these years. As Douglas Hyde put it, "Is Ireland going to be Ireland, or Irelandshire . . . is it going to become a poor little miserable imitation third rate English county?" Delany fought against it but was defeated. Morrissey's study, which is based on material from a wide array of primary sources, is a valuable contribution to our knowledge of a key period in modern Irish history.

HUGH F. KEARNEY
University of Pittsburgh

CHARLES TOWNSHEND. *Political Violence in Ireland: Government and Resistance since 1848*. New York: Clarendon Press of Oxford University Press. 1983. Pp. x, 445. \$39.95.

Many Irish historians focus on particular Irish revolutionary leaders in analyzing the evolution of Ireland's move toward establishing a republic. Charles Townshend focuses on the role violence has played in that revolutionary development. He concentrates on the nineteenth and twentieth centuries, and, although specific revolutionary leaders are mentioned, the central character of this book is not an individual but a force; namely, violence.

Townshend believes that basic to the Anglo-Irish problem is the view each has of the other. The Irish believe the British presence in Ireland is the result of conquest and, hence, that the use of force to rid Ireland of the British garrison is a legitimate activity. The British view their presence in Ireland as that of a civilizing force bringing law and order to the uncivilized. Their reaction to the violence of the uncivilized brings forth comparable acts of "legitimate" violence.

Townshend recognizes that violence is difficult to define. Instead, he concentrates on its effects, and the complexity of violence begins to unfold. He spends much time analyzing various categories of violence such as "competitive," "reactive," and "proactive." He writes at length about how faction fighting, moral force, passive resistance, land hunger, boycotting, and intimidation relate to various aspects of violence. He focuses on the effect those various aspects of violence have on the different levels of Irish society. Townshend thus broadens the reader's view of the role violence has played in Irish history.

Townshend starts with analyses and then moves to relating his analysis to concrete events in history. The revolution of 1848 is touched on briefly. The Fenians, the Land War, and Parnell come next. The earlier sections are interesting, but, as Townshend moves into the twentieth century, the flow of his writing is more compelling. In that latter section he has greater success in blending theory with the events of the day. The reader is thus led into taking

a hard look at the current Northern Ireland problem. The legitimacy of the state appears to be unclear in the minds of many, and the age-old problem of Anglo-Irish violence remains.

Townshend's book is worth reading for anyone interested in Irish or Anglo-Irish history. It focuses on a major theme and broadens the view and, hence, the understanding of the reader. As individuals search for a solution to the problem in Northern Ireland, Townshend's thesis uncovers some major considerations that have to be understood.

BLANCHE M. TOUHILL
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ROBERT DESCIMON. *Qui étaient les Seize? Mythes et réalités de la Ligue parisienne, 1585-1594*. (Mémoires de la Fédération des Sociétés Historiques et Archéologiques de Paris et l'Ile-de-France, number 34.) Paris: Librairie Klincksieck. 1983. Pp. 302. 230 fr.

One would have thought that the Paris Radical or Holy League (*Sainte Union*) had by now become fairly well known. Book by book, a threefold interpretation has emerged. Socially, this ultra-Catholic organization, formed to keep the Protestant king of Navarre away from the French throne, presented itself as an association composed essentially of clerics and members of the lower strata of the legal estate—lawyers and "practitioners"—who composed the *bourgeoisie seconde* (in Henri Drouot's words) and were frustrated in their ambitions for social ascent by the closing down of offices that resulted from the *véralité*. Politically, these extremists were supposed to have erected a party, which I have described elsewhere as "the ancestor of modern totalitarian parties" (*French Historical Review* [1979]). From a more specifically cultural point of view, the league appears "reactionary" in the most neutral sense of the word, that is, as a vigorous response to the centralizing, leveling, and therefore modernizing tendencies of the absolutist monarchy of divine right.

Robert Descimon buoyantly upsets all of our certitudes. The main course that he offers in his book is an amazing prosopographic study of the radical leadership of the Parisian League. According to Descimon's criteria, these "Sixteen," who owe their name to the revolutionary councils set up in each of the sixteen quarters of the capital, numbered, in fact, two hundred twenty-five. Even those who are only slightly familiar with the Parisian archives can fully appreciate the patience and erudition invested in this prosopographic dictionary, a masterpiece of its kind.

Thus armed, Descimon undertakes to show that the blocking of careers, though real, is not by itself a

sufficient explanation for the league. Reconsidering the whole phenomenon, he finds the secret of the *Sainte Union* elsewhere. He locates it precisely in the *union*, that is, in a vision of the social world that presupposes the unity of all sorts of *bourgeoisie* (in the traditional meaning of the word) centered on the privileges of the *bonne ville*. This vision of the city as a community was a "medieval" or, more precisely, a customary vision that stirred up urban society against the pretention of the *officiers* to constitute themselves—through the service of the omnipresent state, which was by definition hostile to local privileges—as a "liturgical order" (in Roland Mousnier's words) detached from and superior to the bourgeois community. On the one hand, the organic society with its bodies, colleges, municipal institutions, confraternities, lineages, and kinships clung with all its strength to its "liberties," or privileges; on the other, individualistic ambitions were put to the service of a global conception of state and society. On one side was the Catholic "zeal" that cemented civic solidarities; on the other was the passion for law and order, authority, and hierarchy.

In Descimon's book, the Holy League appears as the last palpitation of medieval urban civilization under the mortal threat of absolutist monarchy. Ironically enough, its final defeat may well have marked not only the end of the French Middle Ages but also the shelving of the "liberal" potentialities that were to triumph first in England and the Netherlands, before emerging again in revolutionary France.

As is usual in all interpretative attempts that are original and innovative, Descimon's beautiful work raises some difficult problems. For instance, I am still convinced that the partisan aspect (the Holy League as a modern political party) remains essential for the comprehension of the phenomenon and for the explanation of the narrowly social aspect of the radical pledge. Also, I still believe that the author underestimates the role of the clerics in the Parisian League. But does it really matter? Descimon has given us a profound book, admirable for the erudition and creative imagination with which it was written. A magisterial work indeed, *Qui étaient les Seize?* will from now on be indispensable.

ELI BARNAVI

Tel-Aviv University

GEORGE HUPPERT. *Public Schools in Renaissance France*. Urbana: University of Illinois Press. 1984. Pp. xvii, 159. \$19.95.

In this short but provocative study, George Huppert returns to a theme discussed briefly in his book *Les Bourgeois Gentilshommes* (1977). His principal concern is to demonstrate that the hallmark of French

secondary education, a nationwide network of collèges providing uniform education to thousands of youths, should not be credited to the Jesuits, as François Dainville and his followers have erroneously led us to believe. Rather, the collège was a bourgeois creation, the invention of the *gens de bien* who occupied offices in *hôtels de ville* scattered across France.

Huppert's view is that by the end of the fifteenth century the bourgeois were dissatisfied with church schools, which he describes as providing little more than vocational training for boys intending to enter the priesthood. Instead, they sought to establish new schools independent of clerical control that offered a broad classical education and would prepare their children to enter political life, the *vita activa e civile*. Financed from municipal revenues, these schools provided free instruction in French, Latin, and the liberal arts to the sons, and occasionally the daughters, of the local citizenry. City officials selected the teachers personally, generally preferring married laymen with a master of arts degree from Paris. These teachers, many of whom were Protestants, brought to these new schools the "Parisian style" of strictly graded and specialized classes (a total of six), as well as the new Erasmian pedagogy, which taught manners and morality along with the classics and employed a system of praise and blame instead of the whip to foster discipline and learning. The underlying purpose of this program was to teach bourgeois values, which many contemporaries judged dangerous because these ideas accorded greater respect to merit and ability than to social rank. Yet so highly regarded was this particular form of schooling that France around 1600 was the only country in Europe "equipped with a network of colleges . . . entirely owned and administered by municipalities" (p. 110).

Although this last observation is probably exaggerated, Huppert has provided an interesting if somewhat impressionistic overview of the *secondaire* in the sixteenth century, a subject left relatively unexplored in other recent work on the history of French education. His principal sources are little-known monographs published by local historians during the Third Republic, complemented by new manuscript materials drawn from departmental and municipal archives. Of particular value are the analysis of how teachers were induced to leave Paris to teach in the provinces, the discussion of curriculum, and the re-creation of daily routines. Here Huppert imaginatively invokes the bell, its toll signaling both authority and order, as the symbol of the collège.

The book falls short in several key areas, however. The new schools supposedly catered to what Huppert describes as a "new clientele," but the composition of this group is left extremely vague and apparently only amounted to the *gens de biens* who

had the money, as well as the initiative, to keep their children in school well beyond the primary years. Furthermore, if this "new clientele" was so eager to support the new collèges, why is it that, only a few decades after their creation, most of these schools were suffering from inadequate financing, shortages of teachers, and declining enrollments? Huppert attributes this "general crisis in education" to clerical opposition, growing suspicion that the collèges were tainted with heresy, and disruptions caused by the civil wars, together with Henry IV's distrust of schools independent of royal control. This crisis, he argues, eventually led to the demise of these independent schools, as municipalities reluctantly surrendered control to the Jesuits, Oratorians, or one of the other religious orders in an effort to keep the schools open.

This crisis was real. What is suspect is Huppert's analysis of its cause. He hints at a conspiracy; a coalition consisting of the crown, the church, and the religious orders sought to wrest control of the collèges out of the hands of the municipalities and to adapt them to its own purposes. This same coalition is also credited with turning what Huppert sees as innovative and forward-looking institutions of learning into bastions of conservatism: collèges that, in Huppert's somewhat enigmatic and not especially original words, "once had served to produce elites and now chiefly reproduced them" (p. 143). Although it is true that both crown and church in the seventeenth century sought to bring the collèges under some form of scrutiny and control, does not some of the blame for the demise of the independent collège belong to the bourgeois themselves? In Spain, where municipal *colegios* confronted a "crisis" not too dissimilar from that of the collèges in France, local elites often voluntarily gave up control of their schools to one of the religious orders after having found such institutions difficult and expensive to administer. These same elites also found the closely monitored atmosphere of the Jesuits' *internal* far superior to anything that public schoolmasters could offer, whereas the wealthiest families hired private instructors, many of whom offered instruction in new scientific and technical subjects excluded from the classically oriented curriculum of the public schools. Judged from a perspective south of the Pyrenees, it would appear that the "crisis" in French education to which Huppert refers was partly of the bourgeois's own making. It is simplistic to see a conspiracy at work.

RICHARD L. KAGAN
Johns Hopkins University

JEAN-MARIE CONSTANT. *Les Guise*. Paris: Hachette. 1984. Pp. 266. 85 fr.

Visitors to the château of Blois are shown a long room where on December 23, 1588, Henry "Scarface," duke of Guise, was assassinated. Orchestrated by Henry III and carried out by members of the "forty-five," the king's elite body guard, the assassination is described by the guide as a desperate act to prevent Guise from usurping the French throne. Jean-Marie Constant explodes that myth and several others in this splendidly written work, the first full-length study of the Lorraine family to appear in over a generation. He selects specific and controversial events in the sixteenth century to illustrate the complex background of the duel between the two Henrys.

Constant convincingly argues from memoirs of the period and recent scholarship on the Wars of Religion that the hostility between Guise and Henry III dated back to their youth and was compounded by political and religious events that pitted the monarch against the Parisian League led by Guise. The players were engaged in almost soap-opera-style struggles, but on a national and European stage where family antagonisms had far-ranging effects.

There is precious little ideological analysis here. The Guise family is presented in straightforward political, almost Machiavellian, dress. Theology, even religion, is not central to the story. Henry of Guise is portrayed as a feudal prince, a warrior and zealous defender of a medieval ideal of Christian unity, whereas Henry III is the "new monarch," intent on absolute rule and autonomy from the nobility, Rome, and the Gallican church. Guise's success and his eventual downfall resulted from his leadership of the revived League of 1585. Much like Georges Lefebvre's "noble revolt" two hundred years later, the 1588 Day of the Barricades represents the ironic alliance among the old nobility bound by tradition, the bourgeois league, and the third estate, which at the Estates-General meeting of 1588 at Blois sought constitutional reforms that would transform the Valois monarchy into something akin to that of England or Sweden. Henry of Guise appears as a political naïf swept along on the tide of a popular hatred of the king, who in turn felt himself dangerously threatened by these events. Constant avers that both men were driven to extreme action by larger forces, that Guise was not a traitor in the pocket of either Philip II of Spain or the pope, that Henry III believed his mother was too lenient with Guise's desire to rid the court of the upstart favorites, and that, to preserve the Valois concept of monarchy, both Guise and the reform movement of the third estate had to be stopped. Like others of this bloody era, Henry III determined that assassination was the only solution to this crisis.

Written for its popular appeal, with, admittedly, the flavor of a detective novel, this revisionist study will spark closer analysis of the family and its image. Whether guides at Blois will revise their royalist presentations remains to be seen.

JAMES EASTGATE BRINK
Texas Tech University

DAVID BUISSERET. *Henry IV*. Boston: George Allen and Unwin. 1984. Pp. xiv, 235. \$28.50.

David Buisseret's justification for writing yet another book on Henry IV is to provide a general summary of the "information explosion" in recent years on this king and his critical years on the French throne. Henry IV's reign had as profound an influence on the seventeenth century as World War I had on the twentieth, and Buisseret has succeeded admirably. His new biography of Henry IV is a well-written, carefully constructed summary of what is generally known about the king and his reign. Buisseret has, above all, written a story about a man. Without retelling the usual banal anecdotes, the author has imaginatively re-created Henry IV's life and personality and those of his wives, mistresses, and friends. But he has also written an account of how Henry's policies and achievements provided the military, political, diplomatic, and artistic foundations for much of seventeenth-century French government and society.

The book is organized chronologically as is customary in biographies. Buisseret notes in his preface, however, that he has devoted less space than usual to Henry's life before his accession to the throne in 1589—to his military exploits as a Huguenot commander—and more to his activities and achievements as king. He has concentrated on the military events that were critical to Henry's accession to the throne, and these discussions are aided by detailed maps; in fact, the book has a wealth of maps and beautiful illustrations that add greatly to its clarity. Throughout the book Buisseret emphasizes the paradoxical quality of Henry's personality and many of the events of his life, which refuse to fit into a neat pattern. He writes that "everywhere we look, when we look closely, the old easy generalizations collapse, and the reality of things is seen to defy categorisation. But that is how it actually was" (p. xiv). This approach has produced an original interpretation that has the ring of truth about it.

The rest of the book is mostly devoted to an analysis of the major issues and principal actors on the domestic political scene from 1589 to 1610 and explores in particular the impact of international events, especially relations with England and Spain, on Henry's reign. Some of the story is told in excerpts from the English diplomatic correspon-

dence. Much is told in the king's own words. Buisseret has noted in his preface that he tried not to see the reign too much through the eyes of Sully (about whom he has written two books) and to allow the king to speak for himself as much as possible. Many of the chapter subheadings, for instance, are direct quotations from the king's letters, which are the major source for the book. The whole is a succinct, comprehensive summary of the motives and reasons behind royal policy making from 1589 to 1610.

The book concludes with a brief discussion of the intrigues and plots of the nobles that ended in the assassination of the king in 1610. Buisseret does not provide a solution to the problem of who killed Henry IV and doubts that the truth will ever be known. He seems to agree with Roland Mousnier that if Ravaillac had not killed the king someone else would have because of the tense political situation. Foreign agents might have been involved. He concludes that Henry IV's assassination is similar to that of President Kennedy in 1963: both were killed efficiently by social misfits; around both murders swirled many theories not proved or disproved by judicial investigation; in each case there was more than a suspicion that foreign powers were involved; and in neither case was this proved (p. 176).

There are short side excursions into French architecture, military fortifications, royal expenditures, court life, decoration of royal palaces, and provincial government. The breadth of Buisseret's knowledge of the period is impressive, and he has done extensive reading in the secondary literature. That Buisseret spent twenty years writing the book is apparent in its scope and in the careful selection of detail. Buisseret wrote the book for the interested layman and the scholar. Although rarely does a book satisfy both audiences, this one should: it is a highly readable, sound scholarly survey of France during the reign of Henry IV.

SHARON KETTERING
Montgomery College

OREST RANUM. *Artisans of Glory: Writers and Historical Thought in Seventeenth-Century France*. Chapel Hill: University of North Carolina Press. 1980. Pp. xii, 355. \$23.00.

"Seventeenth-century men feared the judgment of future generations almost as much as they feared the Last Judgment" (p. 340). Orest Ranum amply demonstrates that this concluding observation in his book on royal historiographers applies to the Bourbon rulers, their ministers, and the writers paid to record the deeds of the monarchs. Ranum has given us a meticulous and sensitive re-creation of the milieux of the historians and their relationship to the crown. Unlike Nancy Struever, Donald Kelley,

George Huppert, and other recent students of the writing of history in early modern times, Ranum's primary concerns are not styles of rhetoric, the development of the craft of history, or the relationship of historical works to the events they describe. Although these matters are raised in *Artisans of Glory*, the author's chief interest is expressed in the book's main title. It is the interaction of the historians and their royal patrons that Ranum stresses throughout. Thus, this book continues the theme addressed in Ranum's earlier works on Richelieu as ministerial patron and on the development of seventeenth-century Paris under royal and aristocratic patronage. The core of this book is an extended discussion of the state's assertion of control over historical writing and the impact of such controls on the writers.

In part because *Artisans of Glory* stands somewhat apart from other scholarly work on historical writing, this is an easy book to misunderstand. The reader may wonder about the worth of the enterprise because, by the author's own admission, the underlying ideal of royally inspired historiography is "both tautological and banal" (p. 173). Further, the problematic relationship between scholars and the state, an issue that continues to bedevil historians today, cannot help but affect the way we interpret, for example, Jean Racine's use of his gifts to promote Louis XIV's *gloire* and quest for immortality.

Ranum excels at reaching beyond these considerations. He shows that, under the influence of humanist thought, the self-image of monarchy changed in the course of the sixteenth and seventeenth centuries from a Christian ideal of kingship to imperial values drawn from antiquity. The writing of history became a concern of rulers interested in the favorable judgment of posterity. Ranum carefully documents the bureaucratized organization of this impulse in Colbert's patronage of writers, who were paid for recording and celebrating the Sun King's works. The Fronde emerges as the watershed separating the period of multifocused patronage by leading aristocrats, each with a stable of writers devoted to the perpetuation of familial and personal *gloire*, from the latter half of the century, when the crown made itself the most important source of patronage and the center of national consciousness. Ranum argues persuasively that the shift from religious values to a statist concept of French nationalism was exemplified in the crown's employment of writers to compose contemporary history.

Ranum's painstaking scholarship and wide erudition serve him well in his analyses of the published works and manuscripts of the historians Charles Bernard, Charles Sorel, François Eudes de Mézerai, Jean Chapelain, Paul Pellisson-Fontanier, and Racine, each of whom receives chapter-length treatment. The author's many years of immersion in his

subject—he tells us that he began work on this book in the late 1960s—make him an acutely sensitive interpreter of the hidden patterns of personal interaction that shaped the historical discourse. Ranum's own discursive style, which allows for unannounced excursions and for questions posed but often not answered, probably will bother some readers who seek a tidy argument without loose ends. Others may be pleased that, in this study of intellectuals, literature, and politics, the author was true to the ambiguities of the relationship between the state and the pen—ambiguities nearly unchanged since the seventeenth century.

JOSEPH KLAITS
Oakland University

HANS-JÜRGEN LÜSEBRINK. *Kriminalität und Literatur im Frankreich des 18. Jahrhunderts: Literarische Formen, soziale Funktionen und Wissenskonsituenten von Kriminalitätsdarstellung im Zeitalter der Aufklärung*. Foreword by ROLF REICHARDT. (Ancien Régime, Aufklärung und Revolution, number 8.) Munich: R. Oldenbourg. 1983. Pp. xiii, 306. DM 69.

This work takes its place as part of a growing scholarly tradition that seeks to analyze social and intellectual history, or the history of culture, through the medium of the literature of deviance. Perhaps the foremost practitioner of this school was Michel Foucault, but Hans-Jürgen Lüsebrink's book embodies a distinct perspective and an original method. Its point of departure is a corpus of writings known to its Enlightenment audience as *Causes Célèbres*, and its protagonists such as Cartouche, Damien, and Mandrin take their places in the culture and consciousness of Enlightenment France much as Lazarillo de Tormes and Jonathan Wilde did in other European societies at other moments in history.

Lüsebrink brings a potent analytical approach to his subject, one not previously used in works of this genre. As well as providing an exhaustive list of published and unpublished works, he devises a unique schema for penetrating the internal structure of the literature by classifying the works according to format, theme, content, and audience. Not that the reader should expect to be confronted by an endless series of statistics and charts; this is hardly a quantitative exercise in literary analysis. Rather, it is an attempt, largely successful, to render a vast body of literature comprehensible by creating specific categories of analysis, comparison, and contrast.

In devising his analytical categories, the author codifies virtually every thematic element of the texts, particularly those related to criminality and the law. Thus, the literature is scrutinized from nearly every possible perspective embracing the patterns of crim-

inality, the juridical issues involved, the motives of the criminal, the backgrounds of the perpetrator and victim, and the chronology and geography of the criminal acts. Lüsebrink has attempted to devise a classification system within the widest possible context in order to illuminate the full scope of the cultural milieu in which the literature existed and was received. One could quibble with the definition of certain categories (the classification of public authorities is somewhat diffuse) and with the absence of others (types or objects of larcenies), but the analytical system on the whole appears to work.

What emerges from the analytical schema is a method for discerning the range and scope of criminality reflected in the variety and ingenuity of popular culture in that age as expressed through the literary motif of crime. Lüsebrink follows the trail of the *Causes Célèbres* through the formal, didactic landscape (as the *Journal Encyclopédique* and the *Mercure de France*), showing that the expressions of mass consciousness in turn influenced ideology and perception at every level of society. In all, the work is a concise, methodological tour de force in the use of literary, structural analysis to understand the genesis and development of social ideas and attitudes in the preindustrial world.

MICHAEL R. WEISSER
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MAURICE HUTT. *Chouannerie and Counter-Revolution: Puisaye, the Princes, and the British Government in the 1790s*. In two volumes. New York: Cambridge University Press. 1983. Pp. xvii, 268; xi, 269–630. \$99.50 the set.

Maurice Hutt's long-awaited study, a celebration of *événement*, is as much a testament to the resilience of the revolution's defenders as a narrative of failure. It moves with leisure, gravity, and incredible patience to the predictable consummation of the failure, sparing us few of the absurdities that, in the telling, at times seem to overwhelm the countless private tragedies and the larger public disasters that were part of the counterrevolution. Neither commemoration nor execration is Hutt's métier—more than a century of conventional historiography would have in any case ensured against this. But there is an undeniable, though disavowed, tone of judgment, especially in his explanation of the central event in Brittany in 1795 at Quiberon where the chief actors—Puisaye, the insurgents, the emigre and British forces, and, in the background, the princes and the principal figures in William Pitt's ministry—came together to begin the descent into disaster and ignominy, touching off a series of mutually recriminatory accusations and, in the case of Puisaye himself, inducing the first arguments for

an exculpatory memoir of enormous, but hardly decisive, proportions.

Hutt's keen curiosity about the events leading to and following Quiberon has produced a story so filigreed and agonizing in its detail that it threatens to distort our sense of historical time, just as the "Principle of Unripe Time" (p. 512) became the timeless refrain of Puisaye's plaintive appeals for assistance. Hutt's intention to strike the "objective truth," the "strict truth," or "objective reality" almost prevents him from achieving the distance needed to conceptualize the event. He does not, of course, share the actors' belief in the meaning of the event or their illusions about themselves, but the reader may ask if the compulsion to record and verify every bit of information, without a rigorous principle of selection, is not self-defeating because it strains the limits of the narrative form. The urge to know the past in this way can trap the historian and make him its prisoner rather than its interpreter.

Hutt, however, while skirting the abyss, manages to preserve his center of gravity and saves himself from the belief that the narrative grid can totally reproduce the past as either event or structure. He admits that not all the documentation in the world will furnish answers, even if some are now available because of the careful work of historians. What stands out most sharply in this study is his attempt to assess the perceptions of the different sources of discontent in western France. He spends some time in his opening chapter on the heterogeneous and widespread, but often shallow, roots of Chouannerie, and he reveals the ways in which both revolutionary and "royalist" elites worked to gain their submission and loyalty. Hutt makes clear that the wishes of the local insurgents and their more placid supporters, whose "moral anger" (p. 15) against the new regime persisted for years, were not in tune with the goals of those who tried to mobilize them. The antagonism between local insurgency and its self-styled natural leaders—especially outsiders who were as likely as their republican counterparts to think of the locals as savages, gypsies, and Croats—is now an established part of counterrevolutionary historiography, but Hutt might have pushed his exploration further.

Hutt cannot ignore the superior ability of the republicans to gain the support of this volatile population by a well-timed policy of repression and concessions that promised peace and greater scope for normal life and traditional religious rituals. But his major interest is in penetrating the options that Puisaye and the royalists believed were theirs in a period lasting for about five years, starting in 1793. This inevitably raises the questions of wishful thinking, self-deception, and dissimulation as well as of the lack of congruence between the consequences of activities built on such premises and an intrusive,

though not easily decipherable, reality. Events altered fields of vision more rapidly in Whitehall than in Brittany. In Whitehall the debates over the execution of a complex war strategy, encompassing much more than a straightforward policy of assault on Brittany's coastline, were a chronic source of difference between members of the cabinet; in Brittany the deteriorating situation after Quiberon created a mood of desperation and even more fertile ground for deception.

Indeed, the internal politics of the counterrevolution in parts of Brittany must be seen as the response of a social order whose self-esteem was severely damaged by the successful but unpardonable challenges of new men, mainly urban, professional, and commercial, and by the primitive and violent politics of a section of rural society that saw itself outmaneuvered by the better-placed "bigger" men. Puisaye and many of the chiefs who formed a confederation under his leadership shared a highly unstable alliance with bands of Chouans whose mentality and uneven sources of resolve they only partially understood. This lack of understanding resulted not only from the absence of authentic information about the Chouans—information that the British were demanding incessantly and that Puisaye sought—but also from Puisaye's varying capacities to deal with the information. We know from Hutt how nearly unbounded his optimism was, and we may infer that there were significant links between his self-generated enthusiasm, his ambition, his exaggerated belief in his military prowess, and his talent for ingratiating himself with susceptible men like Windham and, momentarily, with Pitt and Grenville.

The golden days did not last long. Puisaye's flaws were primarily neither his indolence nor his *amour-propre* but his taste for the grandiose and his yearning for glory, both of which had little purchase in the changed conditions of revolution and insurgency. His gaze was only dimly affected by their deeper import. He grasped at the only reality he knew. He continued to write letters and to issue manifestos as long as he had the patronage of an indulgent government. What is mysterious about his involvement in the counterrevolution is the near absence of anxiety with which he extricated himself from its misfortunes, but this matches perfectly the ease with which he entered its ranks in the first place. The counterrevolution, however, cannot be reduced to his weaknesses or to those of any other individual. As Hutt tells us, he was never much more than a marginal part of a vaster and longer experience (p. 561). Why he made less of a decisive impact on the counterrevolution than he and his supporters expected and his enemies in royalist

circles feared is now revealed to us because of these two volumes.

HARVEY MITCHELL
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CISSIE FAIRCHILDS. *Domestic Enemies: Servants and Their Masters in Old Regime France*. Baltimore: Johns Hopkins University Press. 1984. Pp. xvi, 325. \$32.50.

SARAH C. MAZA. *Servants and Masters in Eighteenth-Century France: The Uses of Loyalty*. Princeton: Princeton University Press. 1983. Pp. xiv, 368. \$32.50.

Sarah C. Maza and Cissie Fairchilds have produced two excellent works on domestic service in eighteenth-century France. The authors give the reader a sense of perspective in those areas where their studies naturally overlap (Maza provides fullest documentation from archives in Bayeux, Aix, and Marseille; Fairchilds from Toulouse, Bordeaux, and Paris) and raise interesting questions where their interpretations differ. Both give well-crafted profiles of typical male and female servants' careers, based on demographic data, private account books and registers, marriage contracts, wills, tax records, and criminal proceedings. Both offer sophisticated explorations of the mentalities of master and servant, based on the evolving patterns of their interaction in the eighteenth century. Both books include useful tables and well-chosen illustrations.

Maza's study emphasizes the relationship between literary and artistic representation and the subjectively reconstructed experience of the servant. Early chapters on the economic expectations and actual fortunes of servants lead to the core of her interpretation, exploring the triangular relationship between master, servant, and public. In one chapter, entitled "Life on the Threshold," Maza focuses on the predicament of the lackey who, symbolically confined on or near the threshold between house and street, had to while away long hours waiting to be called on some errand by his master. She documents with ingenious detail how the common people perceived servants, how they resented the idleness of the male lackey, and how they assumed that the females were exploited sexually. In another chapter, "The Uses of Loyalty," Maza articulates the notion that the social function of aristocratic service was highly symbolic. The number and attire of male servants, in particular, served as a badge of rank. Servants functioned as intermediaries with the outside world in all situations where physical or face-to-face encounter might otherwise have compromised a theatrically composed and hierarchically organized space.

In a style characterized by a sensitivity to nuance and a rigorous concern for general interpretation, Maza also re-creates the experiences of the typical female domestic. Unlike the male, the female servant almost always regarded her occupation as a temporary stage in her life leading to marriage by means of a respectable dowry accumulated in years of self-sacrifice. Abel Chatelain's article of 1969 serves as a point of departure for both Maza and Fairchilds on this point. Both draw on an abundant and original documentation to portray the obstacles that lay before the *servante* aspiring to better her condition. The young innocent was liable to rape or seduction by master, fellow servant, or tradesman; the older servant was especially vulnerable to betrayal as she began to consider marrying. The differences between the male and female in eighteenth-century domestic service was dramatically illustrated by the revolutionary decree outlawing liveries, those symbols of the estate of male domesticity in aristocratic service. Maza notes the legislators' lack of concern for female servants, whose domestic role as hardworking employees could easily be reconciled with the ideal of a bourgeois household.

Although not ignoring literary and cultural perspectives, Fairchilds draws together a still more comprehensive synthesis of social history relevant to the study of servants, documenting every aspect of servant behavior from thievery to literacy. Establishing an inventory of the various types of sexual relationships involving servants in the household, she examines the effects of these relationships, taken together with other features of eighteenth-century family life, as they shaped the personality of master and servant. Like Maza, Fairchilds stresses the importance of the new emphasis on the bond of conjugal affection in the family from about 1750. Fairchilds corroborates Jacques Depauw's finding for Nantes that master-servant relationships were responsible for a declining percentage of the illegitimate pregnancies declared to authorities in the late eighteenth century.

Fairchilds also pursues Jean-Pierre Gutton's suggestion that the elite's perceptions of the common people were shaped by individual experiences in dealing with servants. In a fascinating chapter, "Servants and Their Masters' Children," she suggests that the upbringing of the seventeenth-century nobleman might be expected to form a personality exhibiting "oral pessimism," whereas the more structured but also more egalitarian relationship between master and servant emerging by the middle of the eighteenth century might have contributed to the liberal mood of the high nobility on the eve of the Revolution.

No student of eighteenth-century society and culture will fail to discover in these two books new ways of looking at the life of the period. For instance, they

provide a new context for reading the *Confessions* of that literate, footloose, but not so utterly atypical *domestique*, Jean-Jacques Rousseau. Differences of emphasis in the two studies will also stir up further questions. Fairchilds seems to view the servant as having been somewhat more integrated in the life of the common people than Maza does. At the same time, Fairchilds discounts the "bridging" role servants might have played between elite and masses. Although both writers conclude that upward social mobility eluded most servants, Fairchilds suggests that many learned "the ways of wealth" and used this knowledge to improve their condition in modest ways. A case in point is that of the great publishing baron Charles-Joseph Panckoucke, whose grandfather, a *valet de chambre*, married a bookseller's daughter.

Both authors demonstrate that the domestication and feminization of the servant profession described by Theresa McBride for the nineteenth century continued trends that originated in the eighteenth century. Maza offers some concluding observations on the nostalgic recurrence of patriarchal values in the advice literature on servants in the nineteenth century. Even if her image of the phobia-ridden bourgeois master is a trifle stereotyped, she provokes reflection by presenting a remarkable passage from *La Nouvelle Héloïse* on the management of servants. Few texts could better announce the tenor of that *société disciplinaire* traced by Michel Foucault. Fairchilds brings her study more firmly to a close with the Revolution, arguing that 1789 is a clean break because masters thereafter ceased to model their perceptions of the lower classes on those they had of their own servants. This assertion, like several others in these two remarkable books, offers a lively challenge to other scholars to examine further the relationship between servants and masters in the broad comparative context of modernization.

THOMAS M. ADAMS

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SUSAN BACHRACH. *Dames Employées: The Feminization of Postal Work in Nineteenth-Century France*. (Women and History, number 8.) New York: Institute for Research in History or Haworth, New York. 1984. Pp. ix, 134. \$20.00.

By the 1980s clerical service has become so stereotyped as women's work that the term "pink collar" is sometimes substituted for "white collar" to symbolize labor that today is generally low-paid, low-status, and dead-end, even if it frequently requires relatively high levels of literacy and skill. Susan Bachrach's slim but sturdy tale of transition in the tertiary sector of the labor market demonstrates that the employment of "lady clerks" once struck observ-

ers as the "immoral and antisocial" act of a "reign of imbecility, the absolute triumph of administrative cretinism" (p. 48), altogether a policy of "féminisation" seen as equivalent to "émasculation" (p. 44). According to Bachrach, however, women were employed as postal clerks to fill new jobs, not to replace men—and, in fact, their employment served to improve employment opportunities for men. Long accepted as post office managers (*receveuses des postes*) in the countryside, the earliest women in the service were widows, wives, or dowryless daughters of privileged families, who tended the public mail at home along with their domestic duties. Change came in the early Third Republic with the conjuncture of expanded postal circulation, enhanced educational opportunities for girls, and new models of independent living for single women in urban areas. An explicit policy of feminization adopted in the early 1890s allowed postal administrators to meet the increased demand for service cost-effectively, while simultaneously solving an "advancement crisis" of men clerks. Introduced in Paris in 1892 in carefully selected residential neighborhoods, postal women, whatever their specific jobs, were all placed in a separate female grade with wages and advancement possibilities restricted by sex. Personnel needs could thus be met without increasing the pool of (male) "auxiliary clerks" already frustrated by a crisis-filled decade of limited promotions. The benefit was three-fold: fewer men filled the bottlenecked lower levels, more men moved up to supervise the expanded (female) ranks, and monies saved by hiring women led to rising (male) wages.

This is not to say that "lady clerks" gained no advantages from their new roles. Postal authorities could justify their departure from custom in humanitarian terms. Educated women, often certificated teachers unable to find appropriate employment, now could enjoy alternative but equally respectable work. Whatever their disadvantages relative to men clerks, women who won admittance to the bureaucracy enjoyed wages and working conditions substantially better than those of most female workers: job security, sick leave, and some chance for advancement within female ranks, along with relatively short hours and high income. Bachrach establishes a solid statistical base for her portrait of the typical clerk as a woman of modest social background who faced an insecure future. Whether emigrant from the provinces or Paris-born, she was most likely the unmarried daughter of an artisan or small shopkeeper and, if Parisian, had a mother employed as a garment worker (26 percent), domestic servant (10 percent), shopkeeper (9 percent), or manual worker (8 percent). Yet those who married—about one-third—almost always found partners in white-collar work.

Bachrach's study is a still-too-rare specimen: a close examination of a neatly delimited population of female workers that shows not only the changing place of women in the labor force but also the role of work in women's lives. Although one may argue, with Michelle Perrot, that it would be "not the Factory but the Office that would consume the Housewife" (in *Romantisme: Revue du dix-neuvième siècle* [1976]), the "intellectualization of women" (p. 61) through employment such as postal service established the ground for some important victories in women's quest for autonomy and equality.

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THOMAS R. OSBORNE. *A Grande Ecole for the Grands Corps: The Recruitment and Training of the French Administrative Elite in the Nineteenth Century*. (Brooklyn College Studies on Society in Change, number 29.) Boulder, Colo.: Social Science Monographs; distributed by Columbia University Press, New York. 1983. Pp. x, 167. \$20.00.

Thomas R. Osborne asserts that, from the Revolution of 1789 to the end of the Second World War, the French never mastered the problem of recruiting and training an administrative elite. And yet he outlines the development of a system that appears to have been extremely effective, if not to his liking. Unfortunately for the reader, Osborne's work is only a brief sketch, and none of the many important issues he raises is given the kind of full discussion (one chapter is a scant four pages) based on archival research that would satisfy a professional historian.

Osborne outlines three possibilities for recruiting *haut fonctionnaires*: (1) use of the auditorat systems established by Napoleon; (2) use of the *licence en droit* from a Faculté de Droit, but with an expanded curriculum for political economy and administrative law; (3) creation of a separate school for public administration similar to the Grand Écoles of the other Grand Corps. According to Osborne, each of these solutions became so identified with a particular political stance that none could be implemented. He then focuses on the École Libre des Sciences Politiques, which did accomplish the task of recruitment and training, although Osborne fails to see it that way.

The author presents generalizations on important areas of investigation, but more work needs to be done before this study can add significant new knowledge. First, we must understand the professionalization of the bureaucracy and its evolving independence from politics. Osborne just sketches how the Council of State, through its control over administrative laws and regulations, built independence into the Grand Corps, especially in recruit-

ment. The council used its system of competitive examinations, the *concours*, for the upper bureaucracy, beginning with the diplomatic corps in 1877 and including all the ministries by the mid-1880s. The founding of the *École Libre des Sciences Politiques* in 1872 was, therefore, well timed to fill a need the law faculties were not filling. Thus, Osborne concludes that through the process of competitive examinations, which could be prepared for only in selective and expensive schools, the social elite of the nineteenth century maintained a key functional role in the twentieth century.

Second, we need more investigation of the development of professional education. Osborne tells us that the *École's* curriculum was dominated by positivism and liberalism: we learn that professors such as Albert Sorel and Elie Halévy taught that order, hierarchy, and individual freedom were key values; that abstract doctrines and radical changes were to be avoided; that political economy had fixed laws that were already known; that the view of society as greater than its individual members was disavowed; and that *supériorité oblige* replaced *noblesse oblige*. The faculty and students of the *École* believed that "real" democracy rested on respect for the "real" superiorities of the elite. Osborne's conclusion that this may have contributed to the "blocked" society—especially since the *École* expanded its curriculum after 1890 in order to meet the private sector's need for higher administrators—is intriguing, but unfortunately here, as elsewhere in this thin volume, Osborne's conclusions are not adequately supported.

THOMAS D. BECK
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CHARLES H. POUTHAS *et al.* *Démocratie, réaction, capitalisme, 1848–1860*. (Peuples et Civilisations, number 14.) Paris: Presses Universitaires de France. 1983. Pp. 617. 240 fr.

Twentieth-century academic historians have had scant interest in devoting their energies to works of global synthesis. This type of writing has been left to the Toynbees, Spenglers, and Durants, whereas for academicians synthesis has been acceptable only if restricted to segments corresponding roughly to the limits of the author's specialty. In the Anglo-American world a number of synthesis series on this model have been launched, but most have foundered as interest in them lagged in recent years. In France, however, the premier synthesis series, *Peuples et Civilisations*, is quite alive, and, now that the research of several generations of historians has appeared in print, it is being substantially rewritten. One of the rewritten volumes, Charles H. Pouthas's *Démocratie, réaction, capitalisme, 1848–1860*, has just

been released. The trio of French historians who collaborated to bring this 1941 work (2d ed., 1948) abreast of the 1980s so reworked the account that the publisher has issued it as a first edition in its own right rather than as the third edition of Pouthas.

Even though the series title indicates this book might be global in scope, it actually focuses on Western civilization with a tilt toward the European and, more specifically, the French perspective. Nonetheless, it briefly treats North and South America, as well as Asia and Africa. The authors see Europe as the "dominant continent," so their emphasis is understandable. The time span covered by this book, a mere twelve years, is surprisingly brief for a synthesis, but it reflects the presumption that this was an enormously important period, as indeed it was. One-third of the book is devoted to an account of the mid-century cleavage, the revolutions of 1848, and the reactions to them. Those potent new forces of liberalism, socialism, and nationalism (as the authors point out, the last term did not enter the *Académie Française's* dictionary until 1835) that inspired the revolutions and drove them forward are aptly described as "evangelical." Although reactionary countercurrents soon prevailed, the authors, in a novel departure from the usual approach, emphasize the permanent changes effected by these "evangelical" forces. Some of the legacies of 1848 noted are the inauguration of democracy in France, an ongoing liberalism and nationalism in Piedmont, the emancipation of the peasants in Central Europe, a schooling in political reality for the peoples of the Austrian empire, and, in Germany and elsewhere, a different mentality brought on by the memory of having participated in a decisive historic event. Even the Prussian three-class system of voting is seen as a concession to the ideal of universal suffrage rather than as a reactionary move. In something akin to a reformulation of the "age of democratic revolution" thesis, the authors argue that most of Europe evolved throughout the 1850s toward "democratic forms."

Although the focus of the book is primarily political, two chapters are devoted to economic questions and one to religious, intellectual, and artistic developments. The authors show that capitalism was viewed favorably by all regimes, whether liberal or authoritarian, and that it touched them all in the 1850s. With the emancipation of the peasantry, technological progress, the railroad and the steamship as new forms of transportation, the Industrial Revolution, and new systems of banking and finance, industry appeared destined to become the dominant mode of production by the 1850s. A disturbingly small amount of the book is devoted to the intellectual, artistic, and literary life of the period and a surprisingly large amount to religious life.

To see how many contemporary forces came to the fore in that brief span of twelve years, it is best to contrast the years 1848–60 with the 1814–48 period and then with our own time. The authors deserve acclaim for pulling together so many diverse threads and for the wealth of detail they provide to reinforce their narrative. Their distillation of general theses is admirable, yet the work seems disjointed because these theses are not integrated into the narrative. Nevertheless, reading the book leaves one with a feeling of mastery over the broad outlines of mid-nineteenth-century European history.

JULIAN ARCHER
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JOSEPH JONES. *The Politics of Transport in Twentieth-Century France*. Montreal: McGill-Queen's University Press. 1984. Pp. xiv, 302. \$35.00.

The bulk of this study of French governmental transport policy deals with the period 1930–50 and focuses primarily on railroads. In the first of these two decades the problem was a growing supply of available transport as trucks and buses entered the market and a contracting demand owing to the Great Depression. The government reacted by restricting truck and bus competition and allowing the railways to cut passenger service, hoping thereby to reduce public subsidies to the railways. This did not work, so finally the state took a majority ownership position in the railways, renamed the Société Nationale des Chemins de fer français (S.N.C.F.), in 1937.

In the 1940s the problem was reversed: war damage and shortages produced a lack of transport facilities, while demand grew, especially in 1939–40 and from August 1944 on. Early in the Vichy period Jean Berthelot, minister of communications, encouraged S.N.C.F. engineers to forget prewar Malthusian policies and prepare a grandiose construction program. They did, but day-to-day reality saw them and the truckers more involved in a desperate effort to move food and fuel from the countryside to the cities. The postwar half-decade saw, at first, reconstruction of railway, canal, and highway bridges and then railway modernization. Joseph Jones finds that toward the end of the 1940s the government's emphasis on the S.N.C.F. as the premier transport agency faded, because trucks and passenger cars became more prominent. He argues that the automobile lobby had become more unified and effective than in the 1930s and that parliament could not forgive the S.N.C.F. for its deficits, despite the public service obligations imposed on it. He finds that since 1950 government policy has become less restrictive on transport of all kinds and the government has adopted a neoliberal stance, hoping

that competition can better serve the public interest than either parliamentary or bureaucratic regulations.

The book is thoroughly researched, because the author exploited the trade press, both national and departmental archives, and the secondary literature. He gets behind the legislation to the pressure groups that influenced it. Therefore, this book is closer to reality than the legalistic study by Kimon Doukas, *The French Railroads and the State* (1945). Nevertheless, it neglects some aspects of French transport policy. It does not deal adequately with canals. The author grossly exaggerates when he asserts that the 1934–36 measures to coordinate railway and road transport halted the automobile industry in its tracks (p. 78). The creation and operation of the S.N.C.F. is treated in so cursory a fashion as to be unclear. The post-1945 role of air transport is rarely mentioned, and the construction of express highways is almost ignored. The book's most valuable sections deal with the complex subject of railway-highway coordination in the 1930s. One may hope that the author will next prepare a thorough examination of the airline and auto route issues just mentioned.

JAMES M. LAUX
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LINDA MARTZ. *Poverty and Welfare in Habsburg Spain*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1983. Pp. xvii, 266. \$49.50.

Brian Pullan and others have already demolished the myth that Protestants had a monopoly on poor relief reform in the sixteenth century, while in Catholic countries the church perpetuated the inefficient and disorganized relief structures of the Middle Ages in order to assure the salvation of the wealthy. Now Linda Martz, in an excellent study based primarily on archival sources, has shown that, contrary to previous assumptions, in both theory and practice Spain too was in the mainstream of efforts to reform poor relief.

Martz centers her study on Toledo, an appropriate place to examine poor relief in Spain. In the sixteenth century it was the second largest city in Castile, it was a major commercial and manufacturing center, its archbishop was primate of the Spanish church, and it was second only to Rome as the wealthiest see in the Roman Catholic world. During the middle third of the century, the population of the city more than doubled, only to drop during the last third, the beginning of a long period of decline for this "Imperial City." This century of boom and bust was marked by increasing pressure on the traditional social welfare structure, and Martz's

book examines the response both of theoreticians and administrators to this challenge.

Martz devotes the first part of the book to the debate in Spain, similar to that in the rest of the Catholic world, between advocates and opponents of poor relief and to efforts throughout Spain, particularly in the post-Tridentine period, to reform charitable institutions. She demonstrates that there was no dearth of proposals and programs for change in sixteenth-century Castile. Perhaps the greatest success was in confinement of the poor; hospitals were established for this purpose, in some of Spain's major cities. But even this achievement was limited; the hospitals were too expensive and did not really work as a means of eliminating public begging and of caring for the poor.

In the second part of the book Martz turns to Toledo itself. She has mined the city's rich archives and effectively exploited a wide variety of sources to present us with a vivid picture of the nature of poverty and attempts to alleviate it. The efforts she discusses range from public programs that outlawed begging and provided systematic poor relief to those of private institutions, such as the great Tavera hospital and the confraternities. She describes in great detail the various relief programs implemented by the city in times of crisis. Martz shows that whether the money for the programs came from the church, the city, or private individuals, they all represented a cooperative effort of the ecclesiastical and secular hierarchy of the city.

This book is a pioneering study on poverty and welfare in early modern Spain. As such, it is a welcome addition to the historical literature not only on Spain but also on poor relief during the Old Regime.

ELLEN G. FRIEDMAN
Boston College

SHLOMO BEN-AMI. *Fascism from Above: The Dictatorship of Primo de Rivera in Spain, 1923-1930*. Oxford: Clarendon Press of Oxford University Press. 1983. Pp. xiv, 454. \$45.00.

This splendidly written monograph on the Primo de Rivera dictatorship deserves a careful reading by a broad audience for at least three good reasons. First, the subject is woefully understudied on both sides of the Atlantic, since scholarly and popular attention have been riveted on the decade of the civil war and its consequences. Recently, Carolyn Boyd observed that the production of studies on the Second Spanish Republic "has become a growth industry," and Stanley Payne mused that the Franco regime has become "possibly the most thoroughly studied dictatorship in the world" (*AHR*, 88 [1983]: 1002). Shlomo Ben-Ami's work amply demonstrates just

how much we have been missing by our failure to study developments in Spain during the 1920s.

A second reason for attending to Ben-Ami's work is that it not only seeks to overcome scholarship's fixation on the civil war era but also promises (on its dust cover) to "help to bring Spanish historiography out of seclusion." Central to the author's interpretation of the Primo de Rivera dictatorship is a comparison of the Spanish experience in the interwar period with the regimes of Pilsudski in Poland, Metaxas in Greece, Stojadinović in Yugoslavia, and Salazar in Portugal. These men were, he contends, "traditional dictators," and, "though they might have started as the champions of a given status quo, they nevertheless approached in varying degrees the threshold of a 'new state'" (p. 397).

This kind of analysis is precisely the third argument for giving this work a careful, even a cautious, reading. Although the title might indicate to the unwary reader that Miguel Primo de Rivera was a disciple of Mussolini, the author argues that this was not the case. Primo de Rivera, the "transitional dictator," seized power in 1923 "in order to defend an antiquated social system against its enemies on the left." He soon realized, however, "that the best defence Spanish capitalism could hope for lay in a certain degree of social change. . . . This was a 'revolution from above'" (p. 401). As Ben-Ami explains, "The Dictatorship had to 'fascistize' itself from above if it wanted to survive" (p. 131).

In some of the monograph's best chapters, the author recalls in detail the transformation of the dictatorship during and after 1924, as the general sought to "civilianize" his administration and locate some mass following for it. In presenting his readers with an account of the development of the Spanish economy during the period, the regime's distinctive attempts at corporatism, and its peculiar regard for the emerging industrial working class, Ben-Ami performs some pioneering work in the field. This is also true of his extensive and very helpful bibliography.

The problem with *Fascism from Above* remains its title and the analytical structure that justifies it. Ben-Ami tells us, "In the view of Italian Fascists, 'Primoderriverismo' collapsed because it was not a coherent dictatorship, or, to put it in other words, it fell short of being a truly Fascist system" (p. 392). Then why employ the label?

DOUGLAS W. FOARD
Ferrum College

R. HAVENAAR. *De NSB tussen nationalisme en "volkse" solidariteit: De vooroorlogse ideologie van de Nationaal-Socialistische Beweging in Nederland* [The NSB between Nationalism and "Folkish" Solidarity: The Prewar Ideology of the National Socialist Movement

in the Netherlands]. Summary in English. (Cahiers over Nederland en de Tweede Wereldoorlog, number 6.) The Hague: Staatsuitgeverij. 1983. Pp. 160.

R. Havenaar's book on the prewar ideology of the Dutch National Socialist movement (NSB) is the sixth publication in a series published by the Netherlands Institute of War Documentation for a general, rather than a scholarly, audience. Havenaar is the author of a biography of Mussert, the leader of the NSB, that appeared in 1978. The present volume centers on the doctrine of the party as a whole, to which Mussert made important initial contributions without dominating the evolution of ideology to the same extent as Mussolini or Hitler did with regard to Italian Fascism and National Socialism.

Havenaar's central thesis suggests that an irresolvable conflict existed between, on the one hand, Mussert's original call for national unity and for a strong and independent Dutch state organized on the basis of the Leader principle and, on the other, the increasing attraction of racial *völkisch* ideology propagated by Hitler's National Socialism. Havenaar rightly claims that these two concepts were basically incompatible, because the acceptance of racial ideology meant that the Dutch, as a Germanic people, would eventually be absorbed by Hitler's Greater Germanic Reich.

This underlying conflict was reinforced by those Dutch National Socialists who advocated the "*Diets*" idea, which envisioned the inclusion of all Dutch-speaking people in a Greater Netherlands state. Such a union of the Dutch with the Flemish on the basis of their common language was diametrically opposed to the National Socialist demand for inclusion of both peoples in a Greater Germanic Reich on racial grounds.

Havenaar also describes the NSB's search for a "usable past." Under the aegis of Groen van Prinsterer and Abraham Kuyper, the NSB claimed as part of its tradition the nineteenth-century Protestant conservative reaction to the ideas of the French Revolution. Although Havenaar correctly points out that the antirevolutionary party accepted the constitutional arrangements of 1848 and proved willing to work within the framework of a constitutional monarchy, he fails to admit that, in a certain sense, the conservatives of the nineteenth century share an intellectual space with twentieth-century fascists in their repudiation of the ideals of the Enlightenment and of popular sovereignty. He is right, however, in suggesting that NSB ideology disregarded the firm historical roots of a commitment to constitutional government and democracy that existed in the Netherlands.

Havenaar portrays adequately the drift during the thirties away from a reliance on essentially fascist

and statist ideas, following the model of Italian Fascism, toward the more dynamic and radical Nazi racist model. He shows that the degree of anti-Semitism embraced by the NSB served as an indicator of the growing radicalization of the movement. In the opinion of this reviewer, the author tends to underestimate the extent to which the NSB under Mussert (or at least the Mussert-wing of the NSB) remained within an essentially fascist mold, despite verbal concessions to the radicals. A comparative perspective, such as a look at parallel developments in the Flemish National movement, might have been illuminating. Havenaar brings out very well, however, the paradox that the NSB, which had started out with the advocacy of a strong national state, ended up by leaning toward an ideology that would have legitimized the absorption of the Netherlands into Hitler's Reich if the Germans had won the war.

WERNER WARMBRUNN
Pitzer College

MERJA-LIISA HINKKANEN-LIEVONEN. *British Trade and Enterprise in the Baltic States, 1919-1925*. (Studia Historica, number 14.) Helsinki: Suomen Historiallinen Seura. 1984. Pp. 312.

This is a much more interesting book than the narrow title suggests. Not the least reason for this is that Merja-Liisa Hinkkanen-Lievonen writes lucidly and compellingly about the international rivalry in the small but important Baltic states amid the chaotic conditions after the First World War. Despite their precarious and unstable existence in the early years of independence, the Baltic states became a center of great interest and intrigue for the major European powers for both political and economic reasons. They were an important source of certain raw materials, they provided a backdoor entry to what was initially perceived as a potentially lucrative Soviet transit trade, and they became an arena in which the great powers sought to contain each other. As the author clearly shows, the political and economic means and ends of this rivalry were inextricably interlocked, and the Baltic may be seen as a microcosm of the power struggles played out in a larger context in the Danubian region and in Eastern and Northern Europe throughout the interwar period.

Ironically, it was Britain, the country with which the Baltic states believed they had closest affinity, that lost out in the struggle for influence in this area. This is particularly true in the economic and commercial field, where Britain was no match for the thrusting Germans. Britain's failure can be attributed to two main factors. First, the poor response of British businessmen and traders to the opportunities available reflected a long-standing problem with

their amateurish methods of conducting business. The second reason was the weak and vacillating foreign policy of the British government toward the Baltic states in this period. After an early burst of enthusiasm arising from the Soviet question, including the trade possibilities that eventually did not transpire, Britain lost interest in the Baltic region, which became a lightweight issue in British foreign policy: "the small role the Baltic states had in Britain's trade policy matched their position in the political field on the fringes of Foreign Office interest" (p. 274).

Whether the Baltic states gained or lost from the interest and intervention of the great powers is a moot point. Hinkkanen-Lievonen is more concerned with the intricacies of the power struggle and rivalry and does not specifically address herself to this question. It would have been interesting to know, for example, whether foreign intervention helped or hindered the task of reconstruction and rebuilding the national life of these tiny states following the traumas of wartime. The study is written very much from the British viewpoint and from British sources and, hence, precludes a fuller analysis of the economic position and performance of the Baltic states in this period. Otherwise, it is constructive and well worth reading.

DEREK H. ALDCROFT
University of Leicester

BENGT NILSON. *Handelspolitik under skärpt konkurrens: England och Sverige, 1929–39* [Trade Policy under Tightened Competition: Britain and Sweden, 1929–39]. Summary in English. (Bibliotheca Historica Lundensis, number 58.) Malmö: CWK Gleerup. 1983. Pp. 217.

Sweden competed with other European states to secure markets during the German-British foreign trade rivalry between the wars. Complicated by depression and Hitler's power politics, trade was a necessity for the Swedish economy, which was dependent on foreign currency and a favorable balance of trade; 30 percent or more of Swedish production went abroad. Likewise, foreign trade was essential to both British and German economic planning. England deserted the gold standard in 1932 and free trade soon after and adopted imperial preference to maintain stability in a chaotic world. British trade policy in Scandinavia shifted to bilateralism, blocked trade, and high-handed methods quite unlike this apostle of free trade. Once Hitler adopted autarchy and sought to dominate Eastern and Southern European markets, it was only a matter of time until German efforts were directed at the Baltic.

Sweden needed to export iron ore, timber products, and finished industrial goods and to import coal, which either England or Germany could supply (Poland undercut both). In the 1930s Swedish policy was to gain stability, whereas England and Germany sought dominance. At the same time, German and British private interests negotiated cartels to lessen competition and divided markets between themselves. Thus, Sweden had to accept British coal to trade in English markets, while arranging some exchange for Germany, which wanted iron ore, forest products, and a few agricultural goods for rearmament and self-sufficiency. It was a brutal struggle for Sweden to maintain balance and stability.

This detailed picture of economic policy shows the lengths to which England and Germany went to preserve economic stability. Appeasement was used long before Munich, with both foreign offices and private leaders negotiating agreements to achieve it. Positioned between the giants, Sweden could only yield (as did the other Nordic states) and make the best possible bargain in a competitive international market. The country sought liberation but gained only a double domination; as the powers rearmed, the scene brightened somewhat because they needed Swedish products.

Bengt Nilson has depicted rivalry and economic policy quite well within a limited scope. He neglects American interests, the Oslo states and their convention, political policies, and international politics. He has kept close to his task but neglects some questions the answers to which would have broadened the scope of the study. He has done well, but reflection would have given greater dimension to an otherwise significant topic in pre-World War II politics and economics.

RAYMOND E. LINDGREN
California State University,
Long Beach

PER G. ANDREEN. *Finland i brännpunkten, mars 1940–juni 1941* [Finland in the Focal Point, March 1940–June 1941]. Köping, Sweden: Lindfors. 1980. Pp. 432.

The sequence of events by which Finland moved from military defeat and diplomatic isolation in March 1940 to cobbelligerency with Germany and renewed warfare in June 1941 has long been discussed. Finnish alignment was a political issue first raised by the prosecution at the trials of wartime leaders in 1945–46. Two extreme interpretations emerged: the "driftwood" theory, asserting that Finnish leaders had no responsible option to German alignment if they were to maintain Finnish independence, and the "revenge" theory, asserting

that they deliberately rejected neutralist alternatives and planned, in conjunction with Germany, an aggressive attack on the USSR.

Scholarly research over the last two decades has added much new information to the debate and has muted the tones of moral righteousness. This progression can be traced in English-language historiography from John H. Wuorinen's early driftwood theory presented in *Finland and World War II, 1939-1944* (1948) through Charles L. Lundin's revenge approach in *Finland and the Second World War* (1957) to Anthony Upton's *Finland in Crisis, 1940-1941* (1964) and H. Peter Crosby's *Finland, Germany, and the Soviet Union, 1940-1941* (1968). Recent research by Finnish scholars has tended to confirm the view that Finnish leaders deliberately sought rapprochement with Germany and were aware that this might lead Finland into war with the USSR on Germany's side. As yet, no contemporary synthesis fully incorporates this newer interpretation.

Per G. Andreen provides what can be termed a limited interim survey. He is a Swedish national who does not read Finnish. He is thus unable to do primary research on many critical points, but he has looked at a large number of the relevant archival and monographic sources in the languages accessible to him. The strengths of his book lie in his efforts to bring together scattered information and to suggest a comprehensive hypothesis.

Andreen argues that Finnish foreign and military policy was shaped by a closed circle of leaders responding to Finnish domestic opinion and the precise chronology of diplomatic relations with the USSR, Germany, and Sweden. Though differing on other points, these leaders shared both an unwillingness to accept the 1940 Moscow peace terms as final and an assumption that Germany would emerge as the military victor in Europe. Their approach to Germany was motivated by the fear of renewed Soviet attack and the hope that Finland could acquire not only the territories lost in 1940 but also chunks of Russian Eastern Karelia. Public opinion in Finland had been nurtured on faith in German military success, and those advocating a *modus vivendi* with Moscow or a neutralist alliance with Sweden were silenced by appeals to national unity. After an agreement in August 1940 that permitted German troop transits in return for weapons, Finnish leaders found their range for maneuver increasingly limited. Andreen argues, however, that Finnish leaders were chiefly limited by their own perception of Germany as strong potential support for Finland in the face of threats from the USSR.

Andreen is least successful in accounting for the public affirmations of peace and neutrality issued by Finnish leaders throughout this period and the lengthy but fruitless negotiations for some kind of

Swedish-Finnish cooperation. Much of this he writes off as a cover-up for the real policy of German alignment, yet at one point (pp. 240-41) he concedes that he cannot entirely reconstruct the "complex game" of Finnish diplomacy.

This is a dense and somewhat repetitive text in which speculation is not always set off clearly from documented argument. It will convey little to readers not previously acquainted with these issues. For scholars familiar with Finnish and Scandinavian events during World War II, Andreen's emphasis on the subjective mind-set of Finnish decision makers as a causal factor in 1940-41 provides an angle well worth consideration.

ROBERTA G. SELLECK
Harvard University Library

HERBERT DAVID RIX. *Martin Luther: The Man and the Image*. New York: Irvington. 1983. Pp. vii, 332.

It is perhaps inevitable that, among the many publications marking the 500th anniversary of Martin Luther's birth, at least one would not be eulogistic or ecumenical in spirit. Herbert David Rix's unflattering portrayal of Luther, so out of step with the times, will probably be dismissed outright or regarded with suspicion and hostility by most scholars.

Many are the reasons they may offer for their opposition to the book. Its author is not a trained expert either in the field of Reformation history and theology or in contemporary psychoanalytic theory. Although he claims to have read widely in the secondary literature, his bibliographical notes and references do not confirm this. His text is also marred by many errors in printing, translating, factual information, and interpretation—indeed, this book bears the marks of a vanity press publication. Disdain for the sanitized image of a heroic Luther proffered by cult biographers has led Rix to concentrate his attention on an examination of the less attractive aspects of the reformer's character and on the psychological bases of his doctrines. If the tools of his inquiry are provided by psychoanalysis, its tone seems to echo the polemics of Johann Dobeneck (Cochlaeus), Heinrich Denifle, and Hartmann Grisar. Rix revels in pointing out contradictions in Luther's doctrinal positions and weaknesses in his character. If many historians doubt the validity of any psychoanalysis of a person who died so long ago, their skepticism may only be heightened when the conclusion is that Luther's theology need not be taken seriously, since it is the product of neurosis.

These objections are not as devastating as they may first seem. Through concentrated effort, Rix has gained a respectable familiarity not only with current psychoanalytic concepts but also with many

of the relevant documents in the extensive corpus of Luther's writings. His lack of sympathy for the reformer has not led him into any major distortions or deliberate falsifications of the historical record, nor are the errors he makes such as to vitiate his insights into Luther's character.

According to Rix, Luther suffered from psychological disorders. Prudently basing his analysis on the documentable symptoms of the reformer's middle and later life, Rix says little about his youth, only that Luther probably failed to form an affectionate bond with his mother, which led him to seek in his adult years neurotic emotional ties with others, and that as a youth he experienced an extreme fear of damnation manifested in his reaction of terror to a painting of Christ the Judge.

To escape this fear Luther sought religious solutions. The promise of eternal life for those who left all to follow Christ led him to embrace monasticism, by which he hoped to escape the wrath of an overly demanding and punishing God for whom he had only hateful and blasphemous thoughts. His efforts to appease this anthropomorphic God by performing good works out of duty not love proved a failure. His next theological solution was the "theology of the cross." Peace for the sinner was to be found in humbly accepting the mysterious will of a hidden God and in embracing divine punishments with love. When this theology also failed to control his pathological anxiety, Luther developed, by a nonrational emotional process, a new psychological technique to meet his deeper needs—the doctrine of justification by faith alone. By accepting as his own the satisfaction Christ made for his sins, Luther was relieved of his need to placate a wrathful God. This solution to his own anxieties Luther now propagated as a scripturally based norm for all Christians (p. 156). Rix shows how Luther was forced either to give peculiar interpretations to texts or to avoid whole sections of the Scripture to protect this interpretation. Thus, the unconscious test of truth was whether a particular position contributed to his psychic requirements. His theology was "an assembly of defensive contrivances clothed in theological terminology" (p. 88). It appealed especially to a variety of fellow neurotics. Any attacks on this theology were perceived as threats to his defenses against the fear of damnation and hence provoked venomous personal counterattacks.

Under the pressure of prosecution for heresy, Luther's neurotic defenses crumbled. He retreated from reality, regressing to infantile impulses, fantasies, and fears. To control these anxieties he developed the typical psychotic symptoms of the manic-depressive and adopted the defense mechanisms of diverting his attention to more pleasurable experiences (companionship, food, drink, and sex), of consciously and unconsciously misrepresenting oth-

ers' positions and accounts of what happened, of engaging in delusions of grandeur and of persecution (his favorite persecutors were a wrathful God, Satan, Moses of the Decalogue, and, especially, the pope), and of resorting to immature, vulgar wisecracks. After a major anxiety attack in 1527, brought on by his fear of recanting his doctrine to escape a violent death, Luther stabilized in a paranoid condition characterized by curses at his arch-enemy in Rome, denunciations of Jews, despotic behavior, and displays of megalomania.

This reconstruction of Luther's psychological development is plausible in terms of psychoanalytic theory and consistent with much of the historical record. Because the data are so fragmentary regarding his youth, Rix's analysis stops short of explaining the deeper sources of Luther's intense fear of damnation. He has thus avoided the pitfalls of Erik H. Erikson's study of Luther's youthful identity crisis (*Young Man Luther* [1958]), which was based on scanty and unreliable earlier evidence. By using additional materials and adopting more recent psychoanalytical concepts, Rix goes beyond the psychiatric monograph of Paul J. Reiter (*Martin Luthers Umwelt, Charakter und Psychose* [1937–41]). Had Rix read widely in contemporary Protestant and Catholic scholarship, he would have given more weight to Luther's scriptural arguments, would not have misunderstood his teachings on good works, the hidden God, and enslaved will, and would have recognized Luther's many achievements. Although Rix criticizes Denifle for being "ill-tempered" (p. 4) in his attacks on Luther, he himself also engages in sarcastic and uncalled-for denunciations of the reformer. Nonetheless, Rix ultimately absolves the reformer of responsibility, because Luther lacked judgment and moral freedom owing to his psychic disorders. It is a pity Rix's insights into Luther's character were not offered free of polemics.

NELSON H. MINNICH
Catholic University of America

R. PO-CHIA HSIA. *Society and Religion in Münster, 1535–1618*. (Yale Historical Publications, Miscellany, number 131.) New Haven: Yale University Press. 1984. Pp. xiv, 306. \$25.00.

R. Po-chia Hsia's thought-provoking study begins where most other studies stop, with the fall of the Anabaptist kingdom in Münster in 1535. Hsia describes the recovery of the city, the restoration of the Catholic ruling elite, and the development of a strong, new Catholicism. The book is based on archival materials, including demographic sources, the *Ratsprotokolle*, tax materials, and wills. Quantitative materials are presented in tables that are models of clarity. An appendix brings together information

on the magistrates of Münster from 1536 to 1618, thus providing scholars with important material on family patterns, wealth, and political position.

The annihilation of the Anabaptist kingdom was a demographic catastrophe: a population of eight to ten thousand was reduced to three to four thousand. Villages and townsmen came in from the countryside, which meant that nearly half of the inhabitants were new to the customs and history of the city. The majority of these new citizens were day laborers, artisans from the humbler trades. Afraid of radicalism, the city forbade the organization of guilds.

The ruling elite was able to dominate political life for two decades, yet there was change. Catholic in faith, this elite divided into patrician and burgher families. Both underwent significant metamorphoses after 1536. The patricians gradually withdrew from civic responsibility, leaving the field to the burghers who were moving from a mercantile to a rentier class. Although appointed by the bishop to their magistral posts, these men were by no means subservient. When the former attempted to construct a fortress inside the walls and provide permanent troops, the burgher elite refused to sign away the ancient liberties of the city.

In 1552 the artisans petitioned the city council for the right to manage their own affairs. The council refused, but the bishop, defeated in battle, had been forced to flee for his life to the city. Welcomed by the artisans who pledged their lives and property in his defense, the bishop moved to restore the guilds and their political institutions. Is this an indication that the early modern city had to bring the artisanal group into decision making?

From 1535 to 1585 there was relative religious toleration. Only Catholic worship was allowed; there is no evidence of an open Lutheran community, but Protestant services were held in private homes. Hsia states that Protestants and Catholics worked side by side in commerce and in the guilds. Furthermore, Catholic worship was heavily influenced by Lutheran practices, including the singing of German hymns and the administration of communion in both kinds.

This ended with the arrival of the Jesuits in 1580, which led to the establishment of a militant Catholicism and the formation of political factions. The Jesuits attracted a new administrative elite—the bureaucrats and jurists of the rising territorial state. The old burgher elite and the guilds now joined to defend the ancient civic liberties and religious toleration. Hsia provides fascinating material with regard to this struggle, the decrease in the use of Low German, and the Jesuit attempt to terminate popular festivals like carnival. Lay wills are examined to demonstrate the degree of civic support for the traditional clergy and the Jesuits.

Hsia concludes that traditional Catholicism served as a civic ideology in the struggle between the burghers and the bishop in his attempt to establish a territorial state. The burghers defended the right of local control. Later, they defended the religion of their fathers against the innovations of the Jesuits. We have assumed that in Protestant cities the support of artisans and merchants came, in part, from their desire to maintain order and the communal ethic of the city. In Münster the artisans, who only arrived after 1536, adopted these same traditional values. The cohesion of the city was their major aim. Hsia's book forces us to look at the similarities as well as the differences between Catholic and Protestant cities. The social, religious, and political aims of the competing groups are brilliantly depicted.

MIRIAM USHER CHRISMAN
University of Massachusetts

PAUL WARMBRUNN. *Zwei Konfessionen in einer Stadt: Das Zusammenleben von Katholiken und Protestanten in den paritätischen Reichsstädten Augsburg, Biberach, Ravensburg und Dinkelsbühl von 1548 bis 1648*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung für Abendländische Religionsgeschichte, number 3.) Wiesbaden: Franz Steiner. 1983. Pp. x, 439. DM 98.

This ambitious monograph examines the possibilities for religious toleration in four south German imperial cities during the so-called century of religious strife. Paul Warmbrunn has assimilated the research of Ernst Walter Zeeden and subsequent investigations of Bernd Moeller, Alfred Schultze, A. G. Dickens, Steven E. Ozment, and Thomas A. Brady to create "building blocks for a still-to-be-written history of confessional formation on the level of the 'common man' between the religious Peace of Augsburg and the Peace of Westphalia" (p. 3).

Three empirical models for peaceful coexistence of rival religious confessions are examined: (1) formal toleration (the passive acceptance of opposing theological creeds), (2) strict parity (the equality—often numerical—of rival confessions within a political and constitutional framework), and (3) the secularized individual religious freedom of the eighteenth century that deliberately overlooked religious distinctions (pp. 4–11). Warmbrunn traces the development of the first two models in the cities of Augsburg, Biberach, Ravensburg, and Dinkelsbühl, where tension between Roman Catholics and Protestants was exacerbated by Charles V's constitutional and political maneuvering, which subjected a Protestant majority in each city to the rule of a Roman Catholic minority. The author wishes to demonstrate "whether and to what extent toleration and civic equality could be realized within the nar-

rowly circumscribed space of a civic community during an age characterized by confessional strife and religious intolerance" (p. 15).

Warmbrunn uses a variety of methodologies taken from both intellectual and social history, but he completely eschews computerized approaches, even though some of his data would lend itself to computer applications. The first half of the book describes the chronological development of religious toleration to 1648; the second half focuses on the socioreligious composition of the city councils and analyzes the influence of confessional rivalry in such civic concerns as marriage courts, education, public morality, and social welfare. A particularly engaging chapter describes the enormous passions engendered by Roman Catholic efforts to introduce the Gregorian calendar in the 1580s, a step that Protestants loudly denounced as a papal plot to undo the entire Reformation.

How successful has the author been in realizing his intentions? That city councils proved to be the ultimate beneficiaries of the confessional struggles will surprise no one conversant with current literature on the subject. One can argue, however, with the rigid and pedantic structure that Warmbrunn imposes on his material—each city is examined separately (but the cities are always in the same order) and is followed by an interpretative summary. This is undoubtedly because the study is the reworking of a doctoral dissertation and would have benefited from more thorough revision. One wishes that the author had restricted his attention to Augsburg alone, for which abundant archival material and secondary studies are available, and had left the other cities for a subsequent journal article.

Although the book's form is simply too complex for the average reader, it should be required reading for those who yearn to impose their particular religious views on virtually every aspect of modern social and political life. Publishing expenses, we are told, were shared by contributions from both diocesan and evangelical treasuries, a touching reminder of how far religious toleration has come, at least in Central Europe!

LOUIS J. REITH
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THOMAS MAX SAFLEY. *Let No Man Put Asunder: The Control of Marriage in the German Southwest; A Comparative Study, 1550–1600*. (Sixteenth-Century Essays and Studies, number 2.) Kirksville, Mo.: Sixteenth Century Journal. 1984. Pp. viii, 210. \$25.00.

For some time now Reformation studies have been shifting away from the grand doings of princes and theologians to the microcosm of the community. This book is a detailed attempt to discern how the

emerging differences between Protestants and Catholics in the sixteenth century affected marriage, a fundamental social institution at once religious and secular. Thomas Max Safley's scope is admittedly restricted. He examines only court records, which by nature report only marital discord gone public; he covers only the half-century between 1551 and 1600; and he studies only three neighboring courts: the *officialatus* of the Catholic diocese of Constance, the general court of the Catholic city of Freiburg in Breisgau (in the same diocese), and the marriage court of the Protestant city of Basel. Two Catholic courts had to be chosen because they divided spiritual and temporal jurisdiction in marriage cases, whereas Protestant marriage courts typically combined both jurisdictions. After the introduction there are separate chapters on the development of marriage law, the activities of each court, the parties to the disputes, and afterthoughts on marriage and the Reformation. Throughout, Safley relies heavily on statistical analysis, partly, perhaps, because the information provided by the case entries in Constance and Freiburg is so laconic as to exclude all other methods.

Safley's findings may be summarized as follows. The *officialatus* functioned almost exclusively as a marriage court (9,997 cases out of 10,283) and as such confined itself to regulation of the marriage contract, in particular the question of the consent of both parties. Although the volume of cases was great, it declined constantly from the 1570s, reaching a nadir of 53 cases in 1610. About 80 percent of all disputes centered on whether a valid agreement between the parties existed in canon law. The court in Freiburg "enforced public morality and marital responsibility as described in local and imperial codes" (p. 121) and was thus usually complementary in nature. To judge from the 721 cases reviewed, what this came down to in practice was magisterial regulation of sexuality, especially adultery. By comparison, the marriage court at Basel was new (1533) and sovereign. It treated questions of both marriage validity and responsibility and accepted cases brought before it by outsiders. In the realm of law, the principal change here, as in other Protestant lands, was the introduction of complete divorce permitting remarriage, whereas the Catholic church continued to insist that, unless a marriage was decreed invalid at the outset, only in certain well-defined cases was a judicial separation possible, which in any event precluded remarriage. Furthermore, the court at Basel viewed marriage not as a sacrament "but as a disciplinary grace, given by God to help humankind avoid the sin of wantonness" (p. 152). The court also stressed formality or publicity in providing the major foundation of marriage and strengthened parental prerogatives (so much for Protestant promotion of individualism!). Safley con-

cludes these case studies by observing that Protestants took a broader view of marital responsibilities than did Catholics. In all three courts, furthermore, the plaintiffs were overwhelmingly women. At Constance they usually sought enforcement of an alleged contract but rarely won; at Basel they often desired divorce and usually got it. Protestant Basel, then, seems to have dispensed better justice to women than Catholic Constance (see p. 179).

Although Safley's handling of this very intricate material is prudent and his conclusions usually sound, questions and objections do come to mind. The putatively broader view of marriage responsibilities taken in Basel may reflect not a new Protestant outlook so much as the division of jurisdiction between Freiburg and Constance, which perhaps accentuated the legal character of marriage and allowed pastoral concerns to fall through the cracks. Safley also fails to explain why the case load fell off progressively at Constance from the 1570s. He offers the hypothesis—which is then sometimes treated as a fact (for example, on pp. 89–90)—that the court failed to satisfy the Catholic laity. That may have been so, but Safley does not go far enough either to prove this or to explore alternative answers (for example, whether Catholic ecclesiastical princes in the area forced their subjects to carry their marriage disputes to their own courts). In general, Safley inclines readily to believe that Catholic courts did not meet the needs of the laity and that Protestant courts did. It is true that by allowing divorce Protestantism made things much easier for people, but Safley is quite wrong to ascribe this either to Protestant fidelity to Scripture or to the inability of the rigid Catholic bureaucracy to adapt to changing social needs (p. 195). Long, long before, the Latin and Greek churches had parted ways on this issue as on so many others. Rome early adhered to Christ's words as recorded in Mark 10:9, which allowed no divorce whatever, whereas Constantinople followed the less severe text in Matthew 19:9, which permitted divorce only on the grounds of adultery. By contrast, the Baslers clearly showed an indifference to Scripture by extending the grounds for divorce well beyond adultery (see p. 131).

Although for its diligence, statistical zeal, and overall carefulness this book certainly merited the Brewer Prize of the American Society of Church History, the marked asymmetry of the courts compared, the limited geographical and chronological scope of the study, the readiness to generalize and the reluctance to discover explanations, and a mild underlying Protestant bias suggest that it must be used with a bit of caution.

LAWRENCE G. DUGGAN
University of Delaware

DAVID WALSH. *The Mysticism of Innerworldly Fulfillment: A Study of Jacob Boehme*. (University of Florida Monographs, Humanities, number 53.) Gainesville: University Presses of Florida. 1983. Pp. x, 139. \$12.50.

Jacob Böhme (1575–1624), the shoemaker of Görlitz, is known for his metaphysical and theological speculations, not his shoes. Nevertheless, his extensive commercial travels spread his ideas. Repeated conflict with the local orthodox Lutheran pastor prompted the city council to suggest to Böhme that he temporarily leave the city. His followers then introduced him to the court circles of Dresden, which further facilitated the spread of his theosophy.

Like other influential figures in the origins of German Lutheran Pietism, Böhme struggled against the strife-prone and divisive rigidity of post-Reformation orthodox confessionalism, which contributed to the outbreak of the Thirty Years' War. It is not surprising that in this context Böhme and others capitalized on the heritage of medieval mystic spiritualism and its expressions in the Radical Reformation to emphasize the necessity for personal and social rebirth and regeneration. Motivated by his own visions and revelations, Böhme incorporated elements of alchemy, Paracelsus, and the Cabala into a metaphysical theosophy that subsequently influenced Pietism, romanticism, and idealism.

This influence on modern thought is the subject of David Walsh's study of Böhme. In three chapters of approximately thirty pages each, Walsh gives an overview of Böhme's "novel construction of reality" and its subsequent intellectual influence, an explication of Böhme's theosophy, and a more detailed exploration of Böhme's "new conceptions of God, the character of evil, and the role of man" (p. 8). A brief conclusion assesses Böhme's significance in the history of ideas and for the present. A good bibliography is included.

Walsh's thesis is that Böhme's theosophical understanding of history is the locus for a definitive modern shift to a political theology of innerworldly salvation. In light of Böhme's influence on Fichte, Schelling, Hegel, and Goethe, among others, this is a thesis worthy of development. Walsh, however, is so captivated by what he sees to be Böhme's contribution to a modern commitment to historical innerworldly fulfillment that Böhme's own historical genesis is ignored if not denied and Boehme's thought becomes the single cause of modern efforts to realize perfection in history. For example, Böhme "provides the essential theoretical means for comprehending the nature of modernity as a whole" (p. 7); Böhme's "crucial transformation of the older philosophic-Christian symbolism into the modern myth

of innerworldly salvation through the dialectical culmination of history" is "*sui generis*" and "has dominated the self-understanding of modern Western civilization" (pp. 22–23).

This is to ignore that enthusiasm in its literal sense of "god-withinism" is as old as religion itself. Indeed, to Luther original sin meant precisely that all persons are born enthusiasts. One Reformation enthusiast in particular was Böhme's compatriot Caspar Schwenkfeld (d. 1561), whose small communities remained in Silesia into the eighteenth century. Likewise, if Böhme is so univocally influential, how would Walsh explain Marxists' perennial fascination for another Reformation enthusiast, Thomas Müntzer (d. 1525)?

Admittedly, a small book with a big thesis cannot explore everything. But Walsh burdens Böhme with more than his real contributions can bear. Too many aspects of historical theology and philosophy are ignored or distorted by Walsh's thesis.

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PETER SCHMIDT. *Das Collegium Germanicum in Rom und die Germaniker: Zur Funktion eines römischen Ausländerseminars, 1552–1914*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 56.) Tübingen: Max Niemeyer. 1984. Pp. xv, 364. DM 98.

The *Collegium Germanicum*, established in 1552, was the first of many papal seminars to educate the secular clergy of a particular nation. Between 1552 and 1914, 5,228 students enrolled at the college, making it the single most influential institution in the shaping of German Catholicism by Rome. Until its dissolution in 1773, the Society of Jesus effectively controlled the college, which itself was forced to close by the revolutionary Roman republic in 1798. The college was restored in 1825 and again came under Jesuit supervision.

Peter Schmidt's work combines the thoroughness of traditional German institutional history with the breadth of a social history approach. The archive of the college and a prosopography of the 5,228 alumni provide the sources for his careful analysis. The work is divided into four parts: an introduction is followed by a concise description of the development of the organization and aims of the college in the context of Roman politics. Although the Jesuits initially emphasized training for pastoral work the most talented candidates regardless of social background, the papal court and cardinals increasingly stressed the cooptation of the German Catholic nobility. By the seventeenth century, sons of noble families who were destined for episcopal dignities became the preferred students at the college.

Part 3 is the heart of the work. It includes quantitative analyses of the students' social and regional backgrounds, academic progress, ordination, and "placement" in cathedral chapters and episcopal stools. An initial period of diversified recruitment was followed by the "ennoblement" of the student body in the seventeenth and eighteenth centuries. Only after its restoration in the nineteenth century did the college attract students primarily from the middle and lower classes. Within Germany, the Rhineland, Westphalia, and the Catholic Southwest sent the largest numbers of students, while Austrian candidates increased in proportion throughout the seventeenth and eighteenth centuries. The importance of the *Germanicum* can be measured by the number of alumni who were elected bishops: between 1560 and 1803, 22 percent of all German and Austrian bishops had been students at the college. Statistical tables and a complete list of the 5,228 students comprise part 4. They offer new sources for more intensive local studies of the Counter Reformation and stand as a tribute to Schmidt's laborious and meticulous scholarship.

Although Schmidt touches on the developments that shaped Catholicism within the German-speaking world, one wishes for a more in-depth analysis of the impact Josephism and the Kulturkampf had on the evolution of German Catholicism. Another shortcoming of the book is the neglect of the content of intellectual and spiritual life at the college, which should complement the detailed analysis and rich material on institutional finances and the curriculum. These minor shortcomings, however, do not lessen my admiration for this most informative study, which is indispensable for furthering our understanding of the social foundations of post-Tridentine German Catholicism.

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FRANKLIN KOPITZSCH. *Grundzüge einer Sozialgeschichte der Aufklärung in Hamburg und Altona*. (Beiträge zur Geschichte Hamburgs, number 21.) Hamburg: Hans Christians. 1982. Pp. 451; 452–813. DM 48 the set.

All authors who have ever chafed under the restraining discipline of a publisher's editor or have ever resented being told to compress this or excise that should consult this gargantuan two-volume work to discover what happens when that discipline is missing. Published by a local history society in its own series and subsidized by no fewer than four sponsors, this book reeks of the self-indulgence bred by the luxury of being able to ignore the marketplace. Its more than eight hundred pages are at least three times too many; its more than three thousand footnotes are at least six times too many.

Franklin Kopitzsch is an exceedingly erudite man. Not only has he read everything there is to be read on Hamburg in the Age of the Enlightenment, but he has also consumed an impressive amount of literature on the European Enlightenment as a whole and many other topics besides. That can be stated with some confidence, because the footnotes are as substantial as they are numerous. They are also as indigestible. On reaching footnote 542 at the end of the introduction, the reader will have acquired sufficient bibliographical information on a multiplicity of subjects to keep himself occupied for the rest of his life—assuming that he is young. On page after page, the footnotes consume more space than the text, which is necessary because many of them include more than a dozen references. Sentences punctuated by four, five, and more separate references are not uncommon. The guiding principle seems to have been: if an event or concept appears, fire bibliographical references at it, the more the better, and never mind if some of them are not on target. The same luxuriant expansiveness spreads into the main text, too, where the argument is interrupted frequently by overly long quotations from contemporary sources and secondary works.

All of this is a great shame, for Kopitzsch has a subject of first-rate importance and both the knowledge and ability to make a lot of it. Persevering readers who are not deterred by the cumbersome form will certainly find much to stimulate and inform them. After describing the political, economic, and social history and structure of Hamburg (and of adjacent but separate Altona), he analyzes the development of the Enlightenment in the city from its earliest stirrings in the first quarter of the eighteenth century to its climax in the last decade. What made Hamburg special was its size (its population had reached about one hundred thousand by 1800), its sustained prosperity, its relative toleration of religious minorities, and its relative openness to foreign cultural influences. Kopitzsch is careful to avoid gilding the lily: economic progress was uneven, social and interdenominational unrest common, religious conservatism tenacious. Yet in the 1720s a secularizing and modernizing movement that can be termed "enlightened" did develop. It was centered on the Patriotic Society founded in 1723 and expressed itself through the weekly journal the *Patriot*, in which discussions and pronouncements on matters previously considered the preserve of the clergy appeared. This section on the theory and practice of enlightened patriotism is particularly interesting.

The movement that developed acquired a character almost unique in Germany owing to the strong participation of merchants. In the second Patriotic Society, founded in 1765, they constituted some 85 percent of the membership. Links with the political

establishment were strong, however, for there was a considerable degree of overlap with membership of the various organs of municipal government. The Enlightenment may never have spread beyond a small minority of citizens, but its relative appeal was such as to give Hamburg a national reputation for intellectual progress. By 1783 the doyen of the Berlin Enlightenment, Friedrich Nicolai, could advise a Karlsruhe theologian to go to Hamburg, because there (and at Zurich) enlightenment and knowledge were diffused more widely than in any other German-speaking city. Hamburg was also both a major consumer and a major producer of literature of various kinds.

The notion of an abridged novel reasonably attracts the charge of vulgarization. An abridged version of this immensely scholarly but unwieldy work would be positively welcome, for its valuable insights and information would then reach the audience they deserve.

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GUNDA MAIRBÄURL. *Die Familie als Werkstatt der Erziehung: Rollenbilder des Kindertheaters und soziale Realität im späten 18. Jahrhundert.* (Sozial- und Wirtschaftshistorische Studien, number 16.) Munich: R. Oldenbourg. 1983. Pp. 210.

The family has been a highly rewarding subject for historians, who once shunned the theme as inappropriate for the discipline. As a pivotal institution in social life, the family is the link to other worlds of work and play and, in the eighteenth century, was still a key to political influence and power. A lively debate in the field concerns the complex and varied forces that have shaped the "modern family." These include new attitudes toward childhood and privacy, expectations of emotional sustenance, and material changes undercutting what in the Germanic world was "*das ganze Haus*," the household that meshed production and consumption. Most historians agree that these changes occurred first among the middle classes and that the values generated by bourgeois experience gradually became normative in the emerging industrial societies of nineteenth-century Europe. Gunda Mairbäurl's study is a welcome and enriching addition to this body of literature. It looks at the role of children's theater in the fortification of the family of the educated bourgeoisie in Germany and the socialization of its young members.

Children's theater in the late eighteenth century was not written for the stage; it was designed to be performed in the privacy of the home. The precondition for this activity, then, was a family that already had considerable wealth and leisure time,

which, the pedagogues argued, would be ennobled by this type of useful play-acting. The literature was consciously didactic (and is thus less interesting for its artistic merits) and functioned to orient young people to their future position in bourgeois society as well as to their grave responsibilities in the family. The plays' subject was life in the family itself, the very world the children were growing up in. They acted out children's roles, experiencing the rewards for virtuous behavior and the punishments for violating the moral codes of their class, but they also played the parts of adults, obtaining a taste of what would be expected of them in the future. Children were socialized into behavior appropriate to their sex and class: boys learned how to deal with girls, girls learned how to behave properly as girls, and both were taught their superiority over the nobles and the lower orders. Parents made up the audience, although they also were given roles in the script so that the whole performance became a shared, intimate experience between adults and their children. This testifies to the emergence of a new relationship among the generations in the family, one no longer based on a shared commitment to work. Mairbäurl connects this literary genre to the broader theme of education, which was one critical ingredient shaping German middle-class self-consciousness and self-worth at the threshold of industrialization.

Since daily life in the educated middle-class family was the subject matter of the theater, Mairbäurl logically asks if the literature mirrored social reality. She handles this tricky question sensitively. The plays' authors (male and now obscure) came from the same social milieu as their heroes and heroines and described life as it was in their own households. For example, the setting reveals a home differentiated into functional rooms, a far cry from the typical "large room" of the past, and the literature conveys much about occupation, clothing, toys, and games. Mairbäurl cautions, however, that the picture of personal relationships (for example, between husband and wife, parent and child) in the plays were as much prescriptive as descriptive and that the literature helped transform the family into an entity based on emotional ties and sensitivities. The family, however, is not undifferentiated but structured by sexual and other relations, and Mairbäurl does not sustain a sensitivity to gender differences throughout the study. Love as the foundation of the new family might have led to greater equality, but this was counterbalanced by bourgeois women's economic dependency on their husbands. Furthermore, I question whether the hackneyed notion of the "apolitical German" is the most useful context for explaining the public concern with the family. A similarly intense preoccupation with the family occurred in England and France, where the

bourgeoisie was not particularly apolitical. Finally, the book is excessively dense and detailed, which detracts from what is undeniably an important contribution.

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KURT G. A. JESERICH *et al.*, editors. *Deutsche Verwaltungsgeschichte*. Volume 1, *Vom Spätmittelalter bis zum Ende des Reiches*; volume 2, *Vom Reichsdeputationshauptschluss bis zur Auflösung des Deutschen Bundes*. Stuttgart: Deutsche. 1983. Pp. xxiv, 941; xxi, 811. DM 258; DM 258.

These two volumes constitute the beginning of a projected six-volume administrative history of Germany. Leadership for the project comes from the prestigious Freiherr-vom-Stein Gesellschaft, which in turn has obtained funding from both the West German government and the Volkswagen Foundation. By the time the last volume is completed, over a hundred scholars, lawyers, and politicians will have contributed articles and expertise. The publication, in other words, is an "official" one and appears with the government's full imprimatur. Indeed, the first volume opens with a foreword by former Bundespräsident Karl Carstens.

A straightforward organization facilitates use of the volumes, edited by Kurt G. A. Jeserich and others. A series of essays devoted to general discussions of early modern administrative problems and, on rare occasions, to interpretative analyses of socio-economic conditions in Germany precedes comprehensive narrative summaries of the expansion of central government in the territorial states. These latter articles focus on particular principalities and in each instance maintain careful distinctions among local, territorial, and imperial institutions. All essays develop chronologically. There is no index in either volume; volume 6 will contain an index for the complete series. Meanwhile, an elaborate table of contents allows for cross referencing by providing precise descriptions not only for the individual chapters, many of which run several hundred pages in length, but also for the numerous subdivisions into which each chapter is divided.

Volume 1 covers the period of the Old Reich, that is, from 1350 to the Napoleonic invasions of the early nineteenth century. The primary focus is on the growth of central government in the territorial states of the empire. The Habsburg, Wittelsbach, Hohenzollern, Welf, and Wettin principalities receive individual attention, as do the states of Mecklenburg and Schleswig-Holstein. Smaller principalities are examined either as generic types (church lands, properties of imperial knights, city states), as

parts of larger administrative institutions like the Swabian Kreis, or as geographic blocs as in the case of the Rhenish and Westfalian territories.

More general essays supplement the specific constitutional studies. These broader, more interpretative articles deal with administrative growth within the Reich as a whole, and several provide socioeconomic generalizations within which the reader can place the more technical constitutional information. There are also discussions of those theories of government that justified expansion by the central administration into areas like education, religion, and the economy. Although imperial institutions receive only cursory attention—the preponderant weight throughout both volumes is on territorial rather than national administration—interested readers will nonetheless find a delineation of the shifting political balance within the Reich and a detailed examination of that most controversial of constitutional formulations, “Kaiser und Reich.”

Volume 2 follows a similar format. It covers the period from the dissolution of the Holy Roman Empire down to the collapse of the Germanic Confederation (Deutscher Bund) and the beginning of Bismarck's bid for a national consolidation under Prussian leadership. Once again, the overwhelming emphasis is on the apparatus of government in the particular states. Almost two-thirds of the volume is given over to this problem. A relatively long opening chapter entitled “State and Society” seeks to place this administrative detail in a broader social, cultural, economic, and political context. But most readers will have difficulty in relating these broad-brush observations to the specific constitutional developments detailed with such intensity in the later portions of the book. Perhaps immediately useful in that regard are the opening chapters, which provide an overview of administrative reorganization after the conquest by Napoleon, a description of the various national assemblies down to 1866, and an account of the steady growth of the German civil service. Although I have no research experience in this area and cannot evaluate the freshness of the scholarship, I also found interesting as a prelude to national unification a chapter dealing with the expansion of a German postal service, the Zollverein, the railroad, and a transterritorial highway system.

Such a project has certain advantages. Although it is unlikely that many English-speaking students will use the volumes, scholars working on broader thematic issues such as nationalism and cameralism may well find these constitutional summaries a ready source for background information that is otherwise difficult to extract. It is important to note, however, that this information is of a very particular sort. The articles offer no more than structural descriptions of a politically fragmented and internally complex geographic area. The broader forces

propelling administrative changes remain a mystery.

In this regard, someone like myself, committed to institutional history, can only regret the series. Wooden, one-dimensional descriptions of administrative growth can only confirm suspicions that studies of government are somehow removed from reality and fail to come to grips with the more important and interesting questions of historical causality. In reading these essays, it is impossible to catch more than the faintest glimpse of the fascinating multilateral bargaining that went on within German society and that propelled administrative growth in the territorial states. Lost as well is the sense of uncertainty and tentative experimentation that characterized the behavior of the contemporary princes, bureaucrats, and estates. No single person or group had even a glimmer of the relentless inevitability that strikes one so forcibly in reading these narrative descriptions. Therefore, those scholars who obtain their background information from this kind of series can only get a distorted setting within which to place their thematic analyses.

The very scope of the project, of course, precludes more than a narrative summary of growth in each instance. I am certain, however, that the particular histories of the institutions could have been placed in a wider, more effective context if the authors had made fuller use of non-German scholarship. In most cases, works by American, English, and French sociologists, anthropologists, political scientists, and historians are simply ignored. Would, for example, the best essay (vol. 1, pp. 388–428) dealing with questions of *Polizei* neglect even a mention of the insights offered by Mack Walker, T. C. W. Blanning, H. C. Johnson, Charles Ingrao, and Marc Raeff? Such omissions are even more glaring in the general and theoretic analyses of society and economics. The most important of such essays dealing with the early modern period (vol. 1, pp. 215–67) contains only four references to non-German scholarship. And, of these four titles, only one is cited in a manner suggesting significant use. This essay, like virtually all the essays in these volumes, also ignored potentially relevant models of the interdependence of economy and society derived from French, British, or American experience.

No country has been more generous with its archival resources or has welcomed visitors more warmly to its libraries than the Federal Republic of Germany. It is extremely discouraging that the work done there by outsiders, as well as the more theoretic works published abroad, have made so little impression on German scholarship.

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HERBERT OBENAU. *Anfänge des Parlamentarismus in Preussen bis 1848*. (Handbuch der Geschichte des Deutschen Parlamentarismus.) Düsseldorf: Droste. 1984. Pp. 815. DM 98.

There was not much "parliamentarism" in Prussia before 1848. Only from that year did the country have a written constitution and a parliament of sorts. Until then the government remained more or less absolutist, although it did produce some protoparliamentary institutions, such as the elected municipal assemblies for which Stein's *Städteordnung* had provided and the set of provincial diets that Hardenberg's successors established in the 1820s. Herbert Obenaus's book is mainly a history of the provincial diets up to and including the joint session in Berlin known as the United Diets of 1847. Surprisingly, Obenaus does not consider the important municipal assemblies, even though he quotes with approval Hugo Preuss's famous dictum that "Die Stadtverordneten waren in der Tat die ersten Volksvertreter in Preussen" (p. 47). This somewhat narrow focus was probably adopted to facilitate concentration on the book's predominant theme, a reexamination of the role of the bureaucracy in Hardenberg's dismissal and in setting the conservative course for Prussian policy thereafter.

Obenaus believes that there was potential popular support for liberal reformism in Prussia in the early years of the nineteenth century and that Hardenberg was trying to bring it into play by insisting that peasant emancipation precede the establishment of a national parliament. The *Staatskanzler* believed that important reforms would be sustained in the long run only by an electorate that extended well beyond the class of large landowners. A conservative cabal within the bureaucracy conspired against him precisely because its members knew he was right. They engineered the establishment of provincial diets based mainly on the landed elite that Hardenberg had wanted to neutralize. The conservatives hoped that these narrowly based institutions would give them powerful assistance in the effort to move the state in a reactionary direction. Thus, on this account Hardenberg appears more liberal and more statesmanlike than he is usually depicted, whereas the effective reactionaries are reduced in number to a few bureaucratic conspirators.

Eventually, the reactionaries had trouble maintaining their chartered course. One of the best parts of the book is chapter 5, in which Obenaus describes how the provincial diets failed to fulfill the conservatives' expectations. Not only did the diets have little impact on public opinion—not surprising, since their deliberations took place behind tightly closed doors and the government censored the announced results so as to expunge any hint of political controversy—but also the landlords' representatives them-

selves turned down many reactionary proposals that were designed to preserve their status because the measures would have diminished the landlords' own ability to dispose of their property freely. Since the diets had only consultative powers, the government often promulgated the rejected laws anyway. But that only served further to undermine the diets' prestige: the government ignored them, so the public did too.

To my knowledge this is the only full-scale analysis of the provincial diets that attempts to place them in their social as well as their constitutional context, and to that extent the book is unique. The story of the United Diets of 1847 is more familiar, so the author has less new material to give us in the later chapters. But he does provide a very plausible account of the liberals' strategy both before and during the United Diets that illuminates regional differences and helps explain the liberals' aggressive quest for a political alliance not with the "people" but with the monarch, a quest that was to be intensified when the revolution broke out a few months later. Since this promonarchical orientation of the liberals is a key element in understanding their actions during the revolution of 1848, Obenaus's account deserves careful attention. The book is a worthy addition to this fine series on the German parliamentary tradition.

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ROGER FLETCHER. *Revisionism and Empire: Socialist Imperialism in Germany, 1897–1914*. Boston: George Allen and Unwin. 1984. Pp. viii, 223. \$29.95.

It is easy to assume that we know most of what we need to know about the political history of the Wilhelmine Social Democratic party (SPD). Social history, which stresses "the class itself" instead of the organizations of the labor movement, encourages this view, but we are still surprisingly ignorant about large areas of SPD practice. Roger Fletcher has now drawn our attention to a glaring example of such ignorance in his bracingly written study of party attitudes toward foreign policy and imperialism.

His book contributes to many different discussions with insight and wit. But his main concern is to correct misconceptions regarding the character of revisionism and to propose the centrality of foreign policy questions for the Wilhelmine SPD. Students of the SPD are long familiar with distinctions—such as those between strict theoretical revisionists, southern German parliamentary reformists, and the so-called practicers in the party and trade union apparatus—but still invariably identify the revisionist theoretical offensive with Eduard Bernstein. For the initiation of certain discussions in the 1890s this is

fair enough. But Fletcher now shows fairly conclusively that Bernstein was an extremely interesting, but idiosyncratic and largely isolated, figure after his return to Germany in 1901, even within the narrower domain of the intellectual right. Instead, the real "impresario of revisionism" was the editor of the *Sozialistische Monatshefte*, Joseph Bloch. Drawing on a wide circle of contributors, including trade unionists, Bloch attempted to build a broad, reformist consensus among the opinion makers of the party. At the same time, he pursued a more specific goal of working-class national and political integration around an avowedly anti-Marxist and imperialist ideology. For most readers Bloch's startling endorsements of a social imperialist perspective will come as a surprise. But in four powerfully argued chapters (including one on Bloch's leading collaborator, Vienna-based Sudeten German Karl Leuthner) Fletcher amply documents the single-minded "realism" of Bloch's intentions in this regard.

Not the least of Fletcher's achievements is to show the indebtedness of both Leuthner and Bloch to the bourgeois cultural heritage of German-speaking Europe rather than to the Marxist tradition. Much the same emerges from his treatment of Bernstein's more "liberal" international perspective in the second part of the book, where he shows Bernstein as increasingly in thrall to the ideas of the British radicals. In the tradition of Gerhard A. Ritter, Erich Matthias, Susanne Miller, and Hans-Josef Steinberg, Fletcher diminishes the importance of the Marxist tradition to the party as a whole while asserting the dubious relevance of party intellectuals and "theory as such" to the ordinary membership. For the foreign policy sphere he has made his case. But he engages in some well-placed polemics against the tendency of social historians to dismiss "formal institutions and ideologies as external inputs and of no importance in the labour movement" (p. 6), which, combined with his own stress on the influence of Bloch, somewhat belies the force of the more general case. If some "formal ideologies" achieved resonance with the rank and file, then why not others? In any event, the popular diffusion of German Marxism has yet to be sympathetically researched, and I am not convinced the last word has yet been said.

In a short review I cannot do justice to the implications, insights, and immense readability of Fletcher's account of revisionism. It is the best we now have.

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HERMANN SCHÄFER. *Regionale Wirtschaftspolitik in der Kriegswirtschaft: Staat, Industrie und Verbände während des Ersten Weltkrieges in Baden.* (Veröffentlichungen

der Kommission für Geschichtliche Landeskunde in Baden-Württemberg, series B, number 95.) Stuttgart: W. Kohlhammer. 1983. Pp. xxxii, 416. DM 42.

Modern German historians have recently discovered the wealth of regional sources, which has led to a veritable revival of the once venerable *Landesgeschichte*. These mainstream historians compete with a quickly growing number of mostly nonprofessional and fringe historians—organized, for example, in the *Geschichtswerkstatt*—for whom the rediscovery of regional history is part of a search for new identities and new politics. Both groups are at loggerheads with each other, but they have begun to produce a noteworthy literature that is reshaping yesterday's modernistic social history. As in much of the nineteenth and twentieth centuries, the new *Landesgeschichte* is the German equivalent to the English history from the bottom up.

Hermann Schäfer's study of Baden in World War I is written in the social science mold. He studies a region in order to elaborate and differentiate national trends. He acknowledges "the stable framework of facts" (p. 1) that is, in his view, provided by national histories. This unquestioned subordination to a national history agenda makes his work occasionally somewhat pedantic. Nonetheless, this book is a splendid achievement, for it opens up a comparatively new field of research and shows this field's usefulness both for social and political history.

Schäfer provides a thorough account of the transformation of economic politics and policies in the state of Baden, which were highlighted by the changes from a "subsidiary" to an "interventionist" state. He describes this transformation against the background of a partly quantitative, partly qualitative assessment of the impact of armaments expenditures on the Badish economy. Although he agrees with others on a number of generally accepted consequences of such a concentration of expenditures, he also points to their wide scope that helped states like Baden improve their relatively backward economies. Naturally, the small number of capital goods industries fared as well in Baden as anywhere else, but, surprisingly, textile industries survived better than one might have expected. Unfortunately, the author does not include agriculture in his study.

The political consequences of the war effort are more central to the study. Although the national mobilization of resources superficially strengthened centralization, it also galvanized local and regional action by bringing reluctant state governments back into economic affairs. By the end of the war, these state governments began to assume a major role. Contrary to the interventionist policies of the central government, the new interventionist consensus on a regional level survived the war. Interventionist pol-

icies flourished because they represented regional economic interests in Berlin. They were, in other words, a result of and an antidote to growing centralization. By 1917 little could be done against the states, which had fought for a fair share in army contracts and had stifled and modified the closure of nonessential enterprises. In 1918 the states demanded a say in demobilization. When the Ludendorff dictatorship broke apart, the German states were more than ever political powers to contend with. Weimar historiography will have to deal with this aspect more than it has done so far.

Although Schäfer does little to study the changing social and cultural configurations in Baden during the war, his portrayal of the contest between the central government and the Badish state over economic policy is a step in the direction of a more diversified and adequate domestic history of World War I in Germany.

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WALTER SCHWENGLER. *Völkerrecht, Versailler Vertrag und Auslieferungsfrage: Die Strafverfolgung wegen Kriegsverbrechen als Problem des Friedensschlusses, 1919/20.* (Beiträge zur Militär- und Kriegsgeschichte, number 24.) Stuttgart: Deutsche. 1982. Pp. 402. DM 58.

During World War I a demand arose within the Allied nations for the punishment of German "war crimes," which included policies such as the violation of Belgian neutrality and unrestricted submarine warfare as well as individual breaches of the rules of war such as the shooting of prisoners. Articles 227–30 of the Treaty of Versailles provided for the extradition, trial, and punishment of guilty Germans. Yet these were the first provisions of that treaty to be revised. Kaiser Wilhelm II remained safe in the Netherlands, and other prosecutions, confined to breaches of the rules of war, were turned over to a German court. Out of an Allied list of 907 alleged criminals, 10 were tried and 4 imprisoned. Six other persons not on the Allied list were later convicted, but the 2 principal offenders escaped (with some help), and their convictions were later quietly reversed.

In the first part of this study, Walter Schwengler reviews the international law of war to 1918. He notes that German legal experts (unlike their English counterparts) usually held that captured soldiers could not be tried for war crimes by their captors. This view (which Schwengler does not share) helps explain why German jurists considered any Allied punishment of German personnel as simply illegal.

But the bulk of the book is on the political history of the punitive articles and deals with broader dif-

ferences in perspective, some of which touch the basic interests and legitimacy of particular groups. British and French leaders had committed themselves to punishing Germany, and Woodrow Wilson was resolved to maintain a united front against the recent enemy. German appeals to Wilson's principles and to Continental concepts of military honor only increased Allied rigidity. But, once the peace was signed, even though under protest, the Allies, and especially the British, wanted a stable Germany.

Allied charges of war crimes threatened the self-image of the old German elites, and many individual officers were in danger of extradition. Yet the new German republic depended on the old officer corps and rightist volunteers for protection against the communists, and—not for the last time—a chancellor became a "prisoner of the Reichswehr" on a major issue (p. 316). German leaders warned the Allies that the military would overthrow any government that attempted extradition, which would probably result in rule by either extreme right or extreme left. The British were open to this argument and unwilling to support French proposals for coercion. So the Allies agreed in February 1920 to test German sincerity by letting Germany try the accused. Then, as Schwengler concludes, "Patriotism and nationalism triumphed over justice" (p. 359).

Unlike James F. Willis (*Prologue to Nuremberg* [1982]), Schwengler says little about particular crimes and nothing about the relation of this post-World War I experience to the Nuremberg Trials. His thorough research clarifies Allied motives and confirms the political importance of the Reichswehr. A good subject index would have greatly improved the book's usefulness. But this study will probably remain for some time as a basic reference work on the politics of war crimes trials.

EDWARD W. BENNETT
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HERMANN WEBER. *Kommunismus in Deutschland, 1918–1945.* (Erträge der Forschung, number 198.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1983. Pp. vii, 187. DM 36.50.

Hermann Weber, the author of this short, useful volume, is probably the most knowledgeable scholar in the West to specialize in the history of the German Communist Party (KPD). Like other volumes in this series, *Kommunismus in Deutschland* is essentially an extended bibliographic essay. Weber's discussion of the abundant specialized literature is set within an outline history of the KPD during the Weimar Republic and the Nazi era. Anyone requiring a quick orientation to the topic will find this book very helpful.

As in his important earlier study, *Die Wandlung des deutschen Kommunismus* (1969), Weber's main concern is to clarify the internal party history of the KPD and detail the wide-reaching transformation of the party's organization, tactics, and personnel that occurred during the 1920s and 1930s. According to Weber, the crucial element in bringing about this transformation was the determined way in which the Comintern asserted its control over the German party and brought about the "Bolshevization" of the party's structure and policies. This intervention was stultifying, Weber insists, and resulted all too frequently in false prognoses of the political situation and mistaken actions.

Weber uses his exceptionally thorough knowledge of the details of party affairs to challenge many of the chief themes that dominate research on party history by specialists in the German Democratic Republic (DDR). Much of the historiographic commentary in the present volume consists of revelations of historical falsification, distortion, and omission in works published in the DDR. In the same vein, he criticizes official biases of DDR scholarship: a susceptibility to mythmaking, an uncritical insistence on the "correctness" of past party tactics, and the modification, from time to time, of the historical record to suit the requirements of the Socialist Unity party's current policy statements. Few Western readers will disagree with Weber on these points.

One could, however, wish for a wider framework of comparative evaluation. Basic questions such as legality or illegality, the pursuit of revolutionary opportunities or the temporary accommodation to parliamentary processes, relations to the Comintern and the Soviet Union, and resistance to dictatorial repression were not unique to the KPD. Would not some discussion of how the Communist parties in Italy, Hungary, and Poland, for example, handled these problems help clarify the special features of the German situation? Even within the context of developments that took place in Germany, Weber's focus on the internal history of the KPD tends to circumscribe his field of vision. A fuller discussion of the influence exercised by communists, either as individuals or through front organizations, on artists, writers, actors, film-makers, and other intellectuals would have been useful. Many well-known leftist intellectuals sympathized with communist causes without becoming party members. The resonance of the communist movement beyond the ranks of the party's organized and disciplined members is clearly an important component of the social and intellectual history of the period.

FRANKLIN C. WEST
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KLAUS J. BADE, editor. *Auswanderer, Wanderarbeiter, Gastarbeiter: Bevölkerung, Arbeitsmarkt und Wanderung*

in Deutschland seit der Mitte des 19. Jahrhunderts. In two volumes. (Referate und Diskussionsbeiträge des Internationalen Wissenschaftlichen Symposiums "Vom Auswanderungsland zum Einwanderungsland?" an der Akademie für Politische Bildung Tutzing, 1982.) Ostfildern: Scripta Mercaturae. 1984. Pp. xiv, 428; xiv, 429–822.

This work contains an impressive collection of more than forty essays, most of which pertain to Germany's place in international migration during the past 150 years. The essays derive from a scholarly conference, held in 1982 at the Protestant Academy for Political Education at Tutzing, that was designed to shed light on current policy toward the five to six million foreigners living in West Germany. With few exceptions these foreigners are "guest workers" and dependents who have arrived during the past twenty-five years from Southern and Southeastern Europe and Turkey. Most have resided in Germany for many years and have no definite plan to return to their native lands, but only a few thousand each year meet the stringent German requirements for citizenship. As several essays document with a wealth of detail, the presence of large numbers of non-German workers in Germany has a long history; by the late nineteenth century, Germany was already shifting from a major exporter to a major importer of labor.

Although the essays achieve their goal of contributing to a better understanding of current West German problems, we would be mistaken to judge the work simply by this standard. The diversity of the contributions and the quality of Klaus J. Bade's editing ensure that these studies can serve many purposes. Most of the essays are by scholars who have published substantial monographs in the fields they analyzed for the conference. Because most of the contributors are German, the collection affords us an opportunity to assess West German contributions to the history of migration within, to, and from Germany. After World War II these subjects were neglected, except by Americans interested in German immigration to the United States and by Germans describing the expulsion of Germans from Eastern and East-Central Europe during the 1940s. Only in the past decade have West German scholars reasserted the traditional German interest in the history of emigration.

Subjects explored in depth at the conference include the international migration of Germans, primarily to the United States, and the situation they found there (contributors include Bade, Hartmut Keil, Willi Paul Adams, Dirk Hoerder, and Kathleen Neils Conzen); the movement of job-seekers within Europe, especially the migration of Polish-speaking subjects of imperial Russia, Austria, and Germany to the Ruhr (contributors include Bade, Christoph

Klessmann, and Toni Pierenkemper); and the economic, social, and legal situation of foreign workers in West Germany since World War II.

Most of the essays offer clear, unobtrusive introductions to the literature and current research on their topics. A good example is an essay by Bade that conveniently pulls together the pioneering work he has done on the German labor market and continental migration during the late nineteenth and early twentieth centuries. Another essay that presents findings from innovative research in primary sources is by the American historian Walter D. Kamphoefner, who scrutinizes the geographical distribution of Germans from different German states within the United States and applies the concept of chain migration suggestively. Criticizing Oscar Handlin's classic work on the uprooting of the European peasant, Kamphoefner argues that emigration served as a way of maintaining (or seeking to re-create) the rootedness of peasant life through the "transplantation" of German villages to another continent.

I was struck by the omission of several topics that would have supplied additional perspectives. The Pennsylvania German migration is excluded, as are German migration to Eastern and Southeastern Europe (which until the 1830s greatly exceeded that to the Americas), German migration to South America, and the migration of Eastern European Jews to Germany. A study of foreign workers in France is included, as is an especially lucid essay on migration from Italy by Peter Kammerer; there is, however, no essay on migrants to or from Britain. Apparently these topics are still neglected by West German scholars.

A recurrent theme is that West Germany has become a country with a large population of de facto immigrants. Two of the specific policy proposals advanced in the volume are at variance with the general tenor of the contributions. Dieter Mertens and Wolfgang Klauder, both civil servants concerned with the labor market, insist that West Germany's "guest workers" must be confronted with a clear-cut choice: to integrate themselves into German society and become citizens or to become true sojourners prepared to leave Germany. This passion to reduce the issue to an either-or solution is curious in the face of abundant evidence in these two volumes that migrants or immigrants seldom see the issue in this way and that economic and social forces rarely permit such stark alternatives.

The experiences of foreign labor in Germany during World War I and under the Nazis are slighted. Of the three essays devoted explicitly to these subjects, two are by East Germans, Lothar Elsner and Jochen Lehmann, who seek to demonstrate basic continuities in labor policies under capitalism before, during, and after the world wars.

Their views are weakly rebutted by West German Klaus Tenfelde in a brief section consisting of excerpts from exchanges at the conference. Few of the other essays have much to say about the distressing subject of coerced, or "slave," labor, even when obviously relevant. Klaus Manfrass refers to hostility toward foreigners in West Germany as a "new phenomenon" (p. 770), as if this hostility had no antecedents in earlier German history. One wonders how the conference greeted Knuth Dohse's suggestion that the West German law of 1965 governing foreigners is a stiffer version of Nazi police regulations of 1938 on the same subject.

The decision to omit most of the transcripts of discussions on the floor was probably wise. The volumes convey a good sense of the debate and controversy surrounding the problems of migration without confronting us with the long, dull, verbatim transcripts West German editors so often include these days in published conference proceedings. We can be grateful to Bade for assuming the formidable responsibility of making available the results of an important conference in a useful, literate, and very readable form.

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PETER STADLER. *Der Kulturkampf in der Schweiz: Eidge-nossenschaft und Katholische Kirche im europäischen Umkreis, 1848-1888*. Frauenfeld, Switzerland: Huber. 1984. Pp. 787. 130 FR.

In Switzerland after 1870, as elsewhere in Central Europe, the hills were alive with the sound of religious conflict. The Roman Catholic Church found itself everywhere beset by the increasingly strident claims of secularism. Given its regional, confessional, and ethnic diversity, the Swiss republic was bound to reflect the general European commotion, of which it was both part and epitome. The many aspects of this complex story have now been gathered into one magistral *Gesamtdarstellung* by Peter Stadler, whose handsome volume surely comes as close to being definitive as any could be.

The work is essentially divided into three parts: a long narrative background of religious controversy in Switzerland from the French Revolution to the Vatican Council of 1870, an analysis of the Kulturkampf during its most confluent phase in the succeeding decade, and an examination of the effects in various cantons throughout the land. Of these, the first and third will be of interest primarily to specialists in Swiss history; the second deserves careful attention by anyone concerned with the central questions of nineteenth-century Europe. Stadler succeeds admirably in integrating his particular

subject with the general development of the time. Specifically, he shows that Switzerland was caught in the intricate triangular webbing of Berlin, Paris, and the Vatican. As one would expect, he finds the German role dominant. For instance, the Old Catholic movement, which revolted against the proclamation of papal infallibility, was "influenced, indeed initiated" by a congress of like-minded dissidents in Munich (p. 373). Other anticlerical currents emanating from Germany in the 1870s seeped through the porous Swiss border and stimulated a series of reactions too consistently parallel to be just coincidental: repressive legislation, police intervention, prohibition of religious services, dismissal of priests, arrests, imprisonments, exiles, and the rest. In striking a balance between mimicry and originality in the Swiss *Kulturkampf*, Stadler stops short of concluding that religious policy in Bern was actually directed from Berlin. Yet the evidence of a penetrating German influence is irrefutable.

Accordingly, Bismarck's shift away from religious persecution after the death of Pius IX in 1878 was necessarily "not without consequences" in Switzerland (p. 553), which thereupon entered a period of appeasement. The one noteworthy exception to that tendency in the early 1880s was inspired by the secular educational reforms of Jules Ferry in France, where a "belated *Kulturkampf*" brought pressure on the church from another source (p. 563). But the federal character of the Swiss state and the irreducible heterogeneity of its population provided Catholicism with a range of opportunities for compromise, thereby forestalling extreme centralization and unduly rigid secularization of schools.

These themes and more are expounded by Stadler with impeccable scholarship based on archival research in London, Paris, Munich, Vienna, and Rome, as well as in a dozen cantonal archives in Switzerland. Withal, his book is a truly impressive performance that merits more than passing notice from those of us who tend to neglect the fate, albeit often so symptomatic, of Europe's lesser powers.

ALLAN MITCHELL
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ÉVA SOMOGYI. *Vom Zentralismus zum Dualismus: Der Weg der deutschösterreichischen Liberalen zum Ausgleich von 1867.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 13.) Wiesbaden: Franz Steiner. 1983. Pp. viii, 119. DM 28.

Éva Somogyi's *Vom Zentralismus zum Dualismus* is really an abbreviated, revised, and amended version of her *A birodalmi centralizációtól a dualizmusig* [From Imperial Centralism to Dualism] that appeared

seven years earlier in Hungarian. Given the Vienna-centered nature of her topic, it is both appropriate and timely for the book to appear also in German, even if in a briefer form.

The Austro-Hungarian Compromise of 1867 and the resulting dualistic system have been a favorite hunting ground for historians, both in the decades following its establishment (G. Beksics, H. Friedjung, L. Eisenmann) and a century later in connection with its centennial (J. Galántai, E. Kovács, P. Hanák, and the works edited by P. Berger, Th. Mayer, A. Vantuch, and L. Holotík). Even so, some aspects of this question remain unexplored. Among them is the role of the Austro-German liberals, whose support of the Compromise of 1867—itsself a compromise of their views—was decisive in the establishment of the dualistic system.

In the first two chapters of the German version of her work, Somogyi lays the foundations of the whole problem by describing Austrian constitutionalism—or rather semiconstitutionalism—as it evolved under Schmerling's leadership during the half-decade following the October Diploma (1860) and the February Patent (1861). She devotes the remaining four chapters to the description of developments between 1865 and 1867 and, more specifically, to the role of the Austro-German liberals in these developments.

One of the important points clearly recognized and emphasized by the author is the ambiguous nature of this role; the German-speaking bourgeoisie were not only liberal politically but also increasingly nationalistic. While demanding a broadly based parliamentary system in Austria, they wished to retain German preeminence in a preferably centralized Austrian empire. This led to a paradoxical situation: the liberals became increasingly opponents, the conservatives increasingly champions, of federalism and of non-German nationality rights.

Given the foreign policy reverses and the domestic problems of the 1860s, the Austro-German liberals were forced to compromise their views by accepting a partnership with the Hungarians. Although disliking the Magyars and detesting this solution, they still regarded dualism—the relinquishment of German control over the empire's eastern half—as preferable to any federalist scheme that would have reduced the German element to one of many coequal nationalities of the Habsburg state. As such, we can agree with Somogyi that the dualistic system, far from being a defeat, was really for the Austro-Germans unavoidably necessary "for the retention of their national hegemony" (p. 111).

Based on much archival material (from Vienna, Prague, Budapest, and Merseburg) and a considerable number of secondary works in half a dozen languages, Somogyi has produced a well-balanced,

scholarly account of a very significant aspect of Austro-Hungarian history that also affected the history of Central Europe for the next half-century.

S. B. VARDY
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WALTER B. SIMON. *Österreich, 1918–1938: Ideologien und Politik*. (Böhlau Zeitgeschichtliche Bibliothek, number 5.) Vienna: Hermann Böhlau. 1984. Pp. 183. S 296.

Walter B. Simon's avowed purpose in writing this book is to provide his students of sociology at the University of Vienna with an inoculation against extremist ideologies, to inculcate in the students a healthy respect for democracy, tolerance, and political pragmatism, as well as to provide them with a liberal perspective on Austrian history in the inter-war years.

The First Austrian Republic collapsed, we are told, because the parties failed to realize that liberal democracy can only function in an atmosphere of nonideological give-and-take. The Austrian parties were too closely identified with specific interest groups and particular regions to be able to serve the common good by agreeing to disagree. Simon suggests that this was in large part because of the influence of outside forces rather than the result of the shortcomings and tensions existing within Austrian society. The Treaty of Versailles was at the root of the problem, for it destroyed the Dual Monarchy and established a number of states that were incapable of survival and that created more problems than they solved. Worst of all, the treaty, by forbidding an *Anschluss* with Germany, denied the Austrians the right to determine their own future. Italian Fascism further polarized the Austrians between sympathizers and militant anti-Fascists. The problem of inflation and unemployment were especially difficult for a country whose traditional economic links were disrupted by the breakup of the empire. The Soviet Union's socialist experiments bewitched and fooled the left, just as Mussolini's success in getting the trains to run on time hoodwinked the right. Finally, Hitler sent the republic packing by invading his homeland.

All this is rather jejune. The attempts to dress the book up with a bit of sociology go little beyond obvious remarks, such as solemnly pointing out that agnostics tended to be socialists and Catholics inclined toward the clericals. Simon greatly exaggerates the effectiveness of Austrian democracy between 1919 and 1934 and overlooks countless cases where politicians of the right hacked away at the foundations of constitutional liberties. The social democrats are chastised for not entering into a coalition with Ignaz Seipel's Christian socialists, even

though such an alliance was clearly out of the question. Engelbert Dolfuss, for example, was most alarmed that the social democrats' offer of support might become common knowledge, and he was unwavering in his determination to destroy the socialists. The dismissal of the Communist party, although it was admittedly quite a pathetic sect, as a bunch of "disoriented and deracinated Jews" leaves an unpleasant taste in the mouth.

There can be no question that "overcoming the past" is a necessary exercise for any healthy society, and Austria needs it as much as any other country. But this can only be done by a thorough and critical examination of the past. Herbert Butterfield's remark that the historian is a witness and not a judge stands at the beginning of the book, but how often this is used to justify a kind of moral relativism and the search for excuses in blind determining forces against which individuals and parties are helpless. Unfortunately, this book is neither a good short history of the First Austrian Republic nor a very convincing account of the failure to create a functioning democracy under conditions that were admittedly far from easy. The enterprise is praiseworthy, but the results are disappointingly meager.

MARTIN KITCHEN
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RADOMIR V. LUZA. *The Resistance in Austria, 1938–1945*. Minneapolis: University of Minnesota Press. 1984. Pp. xv, 366. \$35.00.

At long last, here is a scholarly work on the Austrian Resistance against Hitler. Radomir V. Luza has now added a sequel to his earlier work, *Austro-German Relations in the Anschluss Era* (1975), presenting us with an assiduously researched volume that will take an honorable place in the burgeoning literature on the European Resistance.

In Austria, as throughout occupied Europe, resistance played only a marginal part in the Allied military operations. But the Austrian case had a peculiarity that was crucial and, initially at least, especially debilitating. Since the empire's dismemberment in 1918–19, Austria had been a state without a will to live, its people torn between a sense of Austrian and greater-German identity. This condition explains in part the general frantic euphoria unleashed during the March days of 1938 when Austria's native son returned in triumph to resolve, as it seemed to many, the country's problems.

But the euphoria soon evaporated, and it is often said that the Nazi annexation of Austria gave birth to the Austrians' pride in their separate national identity, which was the precondition for an Austrian Resistance. Luza correctly makes no claim for a centralized resistance movement; at no time was it a

mass phenomenon. But, although he argues that it was "less a national drama than the individual commitment to a personal conscience" (p. 278), he presents the hard facts (more than 100,000 arrests for political acts; 16,493 deaths in concentration camps, 16,107 in Austrian jails, and 9,687 in German ones) and proceeds to identify the groups and tendencies that, occasionally cooperating and separating again, had a cumulative effect on morale in the "Ostmark," as the Nazis renamed Austria, and a distinct impact on postwar development.

The communists clearly emerge, mainly because of their recruitment of former socialist cadres, as the strongest contingent until 1943, after which their apparatus was decimated by the Nazis. Among the noncommunists the "O-5" group, a broad array of politically independent civilians and military men, and the "Traditionalists," growing out of one-time Christian socials, were the leading formations.

The courage of the motley crowd of Austrian resisters was all the more impressive in view of the absence of coordination and the tight Nazi grip on the country. Names like Karel Hudomalj (the Slovene locksmith), the brothers Otto and Fritz Molden, and Major Carl Szokoll of the General Staff might well be recorded in the annals of those who stood fast in times of extreme repression.

Particularly interesting are the contacts between the Austrian Resistance and the outside world. The communists predictably fell under the sway of Moscow. The socialists (that is, the activist revolutionary socialists) envisaged an all-German revolution and cultivated connections across the "border" with friends in Germany. But neither the conservative circles among Austrian resisters nor the former leaders of the mainstream socialists were at all receptive to overtures from the German *Widerstand*. They did not want *Anschluss* even with fellow-German conspirators. In turn, the Moscow Declaration of November 1943, in which the Allied governments committed themselves to reconstructing a free and independent Austria, paved the way for contacts between the Austrian Resistance and the Allies of a kind that the German *Widerstand* never succeeded in establishing. In fact, in January 1945 Fritz Molden obtained from Allen W. Dulles, the American OSS representative in Switzerland, assurance that the U.S. had agreed to give de facto recognition to the Provisional Austrian Committee and O-5 as its Austrian partners.

The Austrian Resistance did not itself bring liberation; after liberation it did not even give birth, as did the resistance movements in all other occupied countries, to a resistance legend. The Second Republic fell back on a party system not much different from that of the First Republic, though no doubt in a very different setting: the new Austria was no longer a state without a will to live, and this crucial

difference owes much to the example that the Resistance gave to its compatriots.

KLEMENS VON KLEMPERER
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M. E. MALLETT and J. R. HALE. *The Military Organization of a Renaissance State: Venice c. 1400 to 1617*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1984. Pp. xiv, 525. \$59.50.

The size and complexity of European armies grew enormously during the fifteenth and sixteenth centuries. Funding, organizing, and controlling that expansion became the first priority of the Renaissance state. In recent years historians have analyzed various aspects of this problem in Spain, France, and England, but few have studied military organization in the smaller territorial states. Venice, as a city-state that evolved into a maritime empire with substantial possessions on the Italian mainland, lay somewhere in between. Its approach to the problem was in some ways unusual, but more effective than contemporary observers such as Machiavelli were prepared to admit.

This book is itself something of a rarity: two fully developed monographs in a single volume. Either could have been published separately. The first two hundred pages, by M. E. Mallett, describe how fifteenth-century Venice developed a standing army out of the much-maligned condottiere system. The Venetians did this primarily by extending the length of mercenary contracts and by granting lands and titles to the largely foreign-born captains, whose interests were thereby more firmly tied to those of the state. Civilian control was maintained through collaterals and proveditors appointed by the senate. Originally intended to serve as little more than a political liaison, these officials, who were invariably Venetian noblemen, became strategic advisors and ultimately achieved control of the support services formerly provided by the captains. The entire process was intricate and accompanied by many vacillations and reversals, but Mallett's treatment is a model of narrative explanation.

J. R. Hale then describes how the system actually worked from the War of the League of Cambrai (1509) to the War of Gradisca (1615-17). The reconquest of Terraferma (the Venetian possessions on the Italian mainland) after Agnadello was a success, whereas Gradisca was in some ways a fiasco, but this is not a tale of decline. These two struggles, and those that occurred in the interim, were different in purpose and character, and their interest, at least from Hale's point of view, lies in the way in which the system responded to a variety of circumstances. Change was confined largely to a steady

increase in the size of Venetian forces, a development characteristic of sixteenth-century armies as a whole, and to the state's growing reliance on subject militias. The latter did not involve theoretical considerations. When, in the second half of the sixteenth century, other Italian states forbade the recruiting of mercenaries within their borders, Venice had no alternative but to rely on her own subjects. Fortunately, they seem to have been well disposed, although the privilege of bearing arms in an increasingly lawless society was perhaps the most powerful inducement for prospective recruits.

The decision to write this book in separate sections has led to a certain amount of redundancy, especially in the description of institutions. It would also be useful to have Hale's admirable description of the city's political system at the beginning of the volume rather than in chapter 10. But these are minor complaints. The matter of the title, which makes the work seem more comprehensive than it actually is, is slightly more serious. The book deals almost exclusively with the military organization of Terraferma rather than with the republic's military posture as a whole. Hale provides a chapter on the fortification of the Empire da Mar, but it is insufficient for a full picture of this larger, more costly area of Venetian concern. The authors make no real effort to deal with the navy, which has, of course, been studied by others, and the chapter on the islands is really something of an intrusion.

Otherwise, *The Military Organization of a Renaissance State* is excellent. The scholarship and writing are of the highest order, and the book abounds in valuable insights on all sorts of subjects, Venetian as well as military. The authors take a revisionist view of the execution of Carmagnola, the significance of the battle of Agnadello, and the reasons for Venetian expansion onto Terraferma in the first place. Their arguments are persuasive. Machiavelli's reputation as a military analyst, already tattered, absorbs a few more shrewd blows, but the "myth of Venice," beloved of eighteenth-century theorists, remains intact in spite of Hale's skepticism. Above all, the book provides a truly superior analysis of the military and political problems of the age. It should be read carefully by anyone interested in warfare, Venice, or the development of the early modern state.

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J. N. STEPHENS. *The Fall of the Florentine Republic, 1512–1530*. (Oxford-Warburg Studies.) New York: Clarendon Press of Oxford University Press. 1983. Pp. xiv, 265. \$42.00.

Here is a proudly conventional study of politics in the late Florentine Republic, an answer to a recent trend that has "dissolved" Florence's political history "into a history of government . . . and society" (p. 1). The first chapter, a review of "modern work" on the fifteenth century, assures us that the experts have bungled the republic's political history, for the "true key" is not "oligarchy" but the "survival" of "popular" government (pp. 7, 9). In the second chapter, J. N. Stephens studies this government in its declining period (1494–1512) and under the initial impact of the Italian wars. Dwelling next on the Medicean hegemony of the years 1512–23 (chap. 3), he also examines the attendant constitutional forms. Chapters 4 and 5 consider the evolution of princely government, the tactical use of Medicean functionaries, the emerging ideology of princely rule, and the erosion of Medici controls after 1523. The concluding chapter treats us to the political bankruptcy and fall of the last Florentine Republic (1527–30).

The achievement of this book is in the archival material of chapters 3 to 5 that concerns the employment of Medicean notaries, the lordly marriages of prominent Florentine families, the pushiness of Medicean henchmen, a scatter of diplomatic negotiations, the way in which the Medici popes (Leo X and Clement VII) put their hands into the Florentine public till, and some behind-the-scenes political hanky-panky. All told, this material brings a more detailed understanding of Medicean government. But the rest of the book, alas, offers us no such virtue.

Stephens's first two chapters miscarry; his story rightly begins with chapter 3 (1512–23). His discussion of the years 1494–1512 in chapter 2 holds nothing substantially new, and chapter 1, his verdict on recent scholarship, is curious, to say the least. He believes that there are "true" and "false" lines in Florentine historiography (pp. vi, 4–13), and the true line is pinned to his notion of the "survival" or persistence of "popular" government. Yet, if this is what politics was "truly" about in fifteenth-century Florence, why factions, why discriminatory taxation, why ingenious and devious electoral ploys, why the bullying of men both inside and outside the political councils, why the practice of illegal vote casting (*a fave scoperte*), why the rise of the powerful *Otto di Guardia* (a combined political police and summary tribunal), and why the mayhem done to the old system of courts?

The weakest features of Stephens's best chapters (3–5) spring from his tacit insistence on a traditional treatment of political history. At the end of such treatment, we long to know far more about the organization of taxation, loans to government (who gained and who lost?), the effects of Medicean policies on the Florentine economy, corrupt court

proceedings, the wide range of political benefits for *seguaci*, and patron-client relations, for all these are vital components of the context for understanding the politics and power of the Medicean domination (1512–27).

But Stephens has more, much more, to answer for in his concluding chapter, where—in a superior and mocking tone—he does not so much analyze as indict the proceedings of the last Florentine Republic. Glancing remarks issue forth here about bigotry, bathos, revenge, the “weeper” spirit, and childlike faiths (pp. 214, 216, 218, 220, 243). No effort whatsoever is made to try to understand why the last republic was pitched into a “frenzy” of moralistic and pious enactments. Stephens’s understanding and sympathies are reserved for the disenchanted and wily patricians, Francesco Vettori and Francesco Guicciardini.

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EDWARD L. GOLDBERG. *Patterns in Late Medici Art Patronage*. Princeton: Princeton University Press. 1983. Pp. xiii, 425. \$50.00.

During the past several decades, historians of art have increasingly concentrated their attention on relationships between producers and consumers of art. This development has been encouraged by the recent emergence of a mass market for works of art and also by the concomitant development and sophistication of the subfield of social history. In any case, patterns of patronage and clientage at times seem completely to overshadow topics like style and iconography that previously dominated studies of art history.

Edward L. Goldberg seeks to develop the newer themes of art history in the case of the Florentine baroque. He deals occasionally with relations between the Medici family and working artists, but primarily with the activities of middlemen who served as negotiating and purchasing agents for their employers. His study relies almost exclusively on correspondence (preserved mostly in the Archivio di Stato in Florence) between Medici patrons in Florence and their various agents in Rome, Bologna, and Venice. The work concentrates on the efforts of Cardinal Leopoldo de' Medici (1617–75) and Duke Cosimo III (1642–1723) as patrons and Paolo Falconieri (1626–96) and his cousin Ottavio Falconieri (1636–75) as procurers of art. Goldberg's documents enable him to throw a surprising amount of light on the seventeenth-century art market, where every type of character from connoisseur to con man haggled over the price and production of works of art, and on the conditions

under which the Medici assembled their collection of paintings and sculpture.

The book exhibits two chief virtues. First, it illustrates admirably the mechanics of the process by which wealthy patrons collected and commissioned works of art. Political developments, social relationships, personnel problems, and a perennial shortage of money—such practical considerations must have influenced the development of many art collections besides that of the Medici. Second, Goldberg offers a thesis (which he might have developed more clearly) that ought to serve at least as a point of departure for similar research efforts on the development of other art collections. The Medici collection, he argues, came together not under the guidance of any principle or theory but rather because of a variety of ad hoc motives, opportunities, decisions, and even accidents.

Despite the book's undeniable virtues, in the final analysis I cannot escape the conviction that Goldberg's work evades the most important issues suggested by his approach to art history. At its best, the study of patronage and clientage contributes to the broader understanding of social history by illuminating the status of artists, the role of patrons, the development of taste, and the evolution of style. Although impressive for its approach and technical qualities, Goldberg's work does not explore the themes that could have raised it to a level of genuine significance.

JERRY H. BENTLEY
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Manoa

VINCENZO FERRONE. *Scienza natura religione: Mondo newtoniano e cultura italiana nel primo settecento*. (Studia e Diritto; Studi e Testi Raccolti da Raffaele Ajello e Vincenzo Piano Mortari, number 9.) Naples: Jovene. 1982. Pp. xv, 701.

The *Principia* made Isaac Newton the greatest figure in European science. Even those who criticized him, with the possible exception of Leibniz, recognized that his stature was greater than theirs. In England he received the adulation of the Royal Society and was consulted by philosophers and theologians as the sole authoritative spokesman of scientific thought. But on the Continent, admiration, however genuine, was initially reserved. Italian scientists, like their European colleagues, clung tenaciously to the Cartesian doctrine of mechanism. Nearly half a century had passed since its first appearance before the *Principia* found a popular champion in France, not in a mathematician but in Voltaire, whose *Éléments de la philosophie de Newton* and *Métaphysique de Newton* were translated into Italian in 1741 and 1742. Of all of Voltaire's works,

these were the most widely read in Italy, and they contributed to the gradual victory and eventual domination of the Newtonian synthesis.

Vincenzo Ferrone defines Newtonianism broadly. After the first chapter in which he surveys initial reaction to the *Opticks* and the *Principia*, he turns in the succeeding chapters to developments in the school of Galileo, the Catholic response to Newtonian theology (or what was perceived as such), the reception of the new science in Venice, the religious stance of Celestino Galeani, and the Enlightenment in Naples. In the concluding chapter Ferrone examines the influence of Antonio Genovesi and his contemporaries in the 1740s.

As the eighteenth century entered its second half, much had no doubt changed in Italy and elsewhere in Europe since the lifetime of Isaac Newton. The new age was less tense and more skeptical, less penetrating and more materialistic than that into which Newton was born. Privately and publicly, men's warmth for religious causes chilled. Devastation simply by hunger was less common than a hundred years earlier. Trade flourished and interest rates declined. The iron barriers of class were beginning to soften. What did Newtonian science contribute to these changes? Directly, very little, but Ferrone shows that Newton and Galileo became symbols of freedom and progress. Their names were constantly invoked to further causes with which they would have been surprised to be associated. From the vantage point that Ferrone selects to survey the opening decades of the eighteenth century, Newtonianism was merely one of the ways the century traveled to the shores of the Enlightenment. The other roads were signposted "Spinoza," "Bayle," "Le Clerc," "Toland," "Collins," and "Locke." From Ferrone's account it would seem that wayfarers often mistook one path for another. Road maps are difficult to read; flag-waving is more fun!

WILLIAM R. SHEA
McGill University

ANGELO TAMBORRA. *Garibaldi e l'Europa: Impegno militare e prospettive politiche*. Rome: Ufficio Storico Stato Maggiore dell'Esercito. 1983. Pp. 251. L. 9,000.

The centennial of the death of Giuseppe Garibaldi (1807–82) occasioned numerous publications in his honor. This book is a noteworthy example. Angelo Tamborra, a specialist in military history, has pulled together eight essays based on his research in sources that are not easily accessible to most scholars. On balance, it is a respectable contribution. The theme of many chapters is Garibaldi's sometimes romantic but nevertheless sincere interest in the various movements for national self-determination in East-Central Europe and the Balkans—an ideal-

istic concern that he repeatedly had to lay aside in the face of the harsh military and diplomatic realities confronting the young Italian state.

Tamborra is conversant with numerous languages and has drawn frequently from sources in Polish, Serbo-Croatian, and Czech, among others. The book brims with names of foreigners who either volunteered to fight alongside Garibaldi in his Italian campaigns or hoped to elicit his military support for their own causes of national liberation. The study is therefore a useful reference tool; unfortunately, the author has failed to take advantage of an opportunity to provide a more systematic clarification of the international and domestic context in which these nationalistic factions were moving. Tamborra focuses his attention strictly on those foreigners who were caught up in the magic spell of Garibaldi. He calculates that at least two thousand foreigners volunteered to join Garibaldi's Thousand in 1860, but he does not mention the foreign soldiers who fought in Italy on the other side of the battlefields.

In a long essay, "The Example and the Expectations," Tamborra argues that, before the defeat at Aspromonte in 1862, Garibaldi toyed with the possibility of launching incursions into Greece, Albania, and Montenegro. Tamborra points out, too, that during the tragic Polish revolt of 1863–64 numerous Magyar patriots hoped to gain Garibaldi's support in a military pincers move against Austria. This idea was rejected as impractical by General Lamarmora and the Italian government.

In "Fidelity to 'Crucified' Poland and to France, Country of 'Immortal Principles,'" Tamborra discusses Garibaldi's long-standing interest in the Polish question and his contacts with Polish emigres. This is followed by a summary of the remarkable military role that Garibaldi played as head of the Army of the Vosges, fighting on behalf of the newly proclaimed Third Republic in France during the war against Prussia in 1870–71. Another essay, "Garibaldian Interventions in the Balkans, 1867–1916," chronicles little-known activities in Bosnia-Herzegovina, Albania, Greece, and Crete on the part of some Garibaldians, including the liberator's son, Ricciotti. The interest aroused by Garibaldi among certain Russian populists—Alexander Herzen, Michael Bakunin, and Sergei Kravchinskii (Stepniak)—provides the topic for still another chapter.

In an interesting essay, "The Ideal of Peace and Internationalism," Tamborra discusses Garibaldi's cosmopolitanism and his humanitarian desire to seek remedies that might have saved Europe from its periodic conflicts. He points out that Garibaldi favored a European confederation modeled on that of Switzerland, and he gives special attention to the Congrès pour la Paix et la Liberté, held in Geneva

on September 7, 1867, where such men as John Stuart Mill, Victor Hugo, and Herzen participated. To the dismay of some in the audience, Garibaldi used this podium to attack the papacy.

In his old age, Garibaldi considered socialism as a possible means for promoting humanitarian solidarity. In this regard, Tamborra's discussion of the meeting of the Russian anarchist Bakunin with Garibaldi in 1864 is informative. Although it soon became clear that the two men's views were incompatible, it was, ironically, Garibaldianism that enabled Bakunin's ideas to make some headway in Italy during the next decade.

Tamborra concludes his essays by summarizing the evaluations of Garibaldi that were expressed in 1907 by Karl Kautsky, Maxim Gorky, and others on the centennial of the liberator's birth.

CHARLES F. DELZELL
Vanderbilt University

MARTA PETRICIOLI. *L'Italia in Asia Minore: Equilibrio mediterraneo e ambizioni imperialiste alla vigilia della prima guerra mondiale*. Foreword by ENNIO DI NOLFO. Florence: Sansoni. 1983. Pp. 478. L. 32,000.

Marta Petricoli has produced an exhaustive, virtually day-by-day reconstruction of Italy's would-be imperialism in the eastern Mediterranean on the eve of World War I. Although one longs for the color and humor of more general, English-language accounts (for example, those by Richard Webster and R. J. B. Bosworth), this book will be difficult to match as a study of both Italian relations with the faltering Ottoman empire and the complex mechanics of Italian expansionism at the time.

The attempt to create a sphere of influence in southern Anatolia involved a dizzying variety of characters on the Italian side. The author has made systematic use of the archives of the president of the council of ministers, the foreign ministry, the navy, the Bank of Italy, the Ansaldo Company, and Bernardino Nogara, the Banca Commerciale's agent for Turkey. Italy's sometimes mysterious activities and intentions were a source of considerable alarm and contention among the other powers in the tense atmosphere of 1912-14. Petricoli has drawn on the German, Austrian, French, and British diplomatic archives to show how Italy's eastward expansion figured in the breakdown of the Triple Alliance and in relations between the Triple Alliance and the Triple Entente.

Several basic themes emerge. None of them will startle those familiar with the period, but they are based on an astonishing collection of factual detail. Italy's, and in particular the Marquis Di Sangiuliano's, determination and inventiveness in playing the imperial game were matched only by the paucity

of material resources. Diplomatic finesse, blackmail, and legerdemain in the end did not make up for a lack of economic and military power, although one cannot help but admire the energies of this Sicilian Don Quixote.

The old question whether business followed the flag or vice versa seems irrelevant in the case of southern Anatolia. The foreign ministry and Banca Commerciale moved more or less in tandem, pursuing distinct though complementary goals. Sangiuliano was moved by the simple conviction that great powers had to behave as such to command respect and that Italy must have a piece of the Ottoman empire like everyone else. Nogara sought export business for firms tied to the Banca Commerciale and a larger piece of the Turkish public debt. Sangiuliano's designs required business penetration, Nogara's needed diplomatic support.

In the end, neither side had much to offer the other. The Comit's German connections delivered little from Germany and eventually caused problems with Italian public opinion and the French. Sangiuliano's policy of "small *faits accomplis*" followed by negotiations produced only a tenuous foothold on the Turkish coast, serious tensions with Austria, and a bad example for future Italian diplomacy.

This book is to be recommended as a model of archival scholarship. It could use some maps and, like most Italian publications, a decent index.

JOHN L. HARPER
Johns Hopkins University

ROBERT BLOBAUM. *Feliks Dzierżyński and the SDKPiL: A Study of the Origins of Polish Communism*. (East European Monographs, number 154.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1984. Pp. viii, 307. \$30.00.

This well-researched monograph by Robert Blobaum focuses on the pre-Bolshevik career of Feliks Dzierżyński, a Pole who joined the Bolsheviks in August 1917, became the first head of the Cheka, and, in general, occupies a prominent place in the modern revolutionary pantheon.

A cursory introduction provides an appropriate setting for understanding both Dzierżyński and the revolutionary Marxist party, the Social Democracy of the Kingdom of Poland and Lithuania (SDKPiL), which he helped found with Rosa Luxemburg, Leon Jogiches (Jan Tyszka), and others. Nine chapters of narrative and analysis follow. Blobaum first explores Dzierżyński's Polish gentry childhood and adolescence and his political evolution into a socialist revolutionary. He then concentrates on the dominant role the young Dzierżyński played, especially

between 1899 and 1912, in shaping the organizational structure and determining the political strategies of the SDKPiL, operating mainly in the Russian zone of partitioned Poland. In December 1918 this party merged with the PPS-Left to create the Communist Workers' party of Poland.

Blobaum's monograph is based primarily on two principal collections in the Central Archive of the United Polish Workers' Party in Warsaw. These extensively cited sources are supplemented by a broad range of English, German, Polish, and Russian primary and secondary materials, although, among the latter, George Leggett's recent publication on the Cheka is omitted. An index, bibliography, and illustrations also are included.

Blobaum proposes that Dzierżyński, who became an obsessive revolutionary, was the mainspring of the political organization of the SDKPiL, the key figure in its collective leadership, and therefore the founder of what would become the Polish Communist party (p. 5). He handles his sources skillfully, cautiously weaving into the text carefully documented criticisms of shortcomings in the existing studies of his subject—both communist (Soviet and Polish) and Western—and he effectively undermines numerous myths inherent in them. For example, he argues that Dzierżyński was not a Leninist before 1917, although Dzierżyński's obsession with revolution was in many ways analogous to Lenin's. Like Lenin, Dzierżyński devoted his energies to creating a highly centralized party, and by 1905 the SDKPiL began to resemble Lenin's Bolsheviks (p. 120). Blobaum amply substantiates that a variety of major influences, other than Lenin, contributed to Dzierżyński's organizational and ideological outlooks. The most important of these included: his personal experiences in prison and exile and as a clandestine agitator and organizer; the Jewish Bund; and Rosa Luxemburg's Marxism. Dzierżyński's "Luxemburgism" and his political organization carried over from the SDKPiL to the Polish Communist party.

Such cautious demythification and judicious analysis of archival evidence results in a realistically dispassionate political portrait of Dzierżyński, a complex revolutionary personality. Blobaum's study also contributes significantly to the reader's understanding of Dzierżyński's role in the development of Polish communism and provides a thoughtful prologue for anyone interested in Dzierżyński's formidable career as a Bolshevik after 1917 until his death in 1926.

This monograph, despite its attributes, also contains several editorial flaws. Notably, misspelled words abound. Such carelessness detracts from the text and is inexcusable in a scholarly work.

JOAN S. SKURNOWICZ
Loras College

SHMUEL KRAKOWSKI. *The War of the Doomed: Jewish Armed Resistance in Poland, 1942–1944*. Translated by ORAH BLAUSTEIN. Foreword by YEHUDA BAUER. New York: Holmes and Meier. 1984. Pp. xii, 340. \$37.50.

The nightmare lasted just a few years, but its wounds will linger for centuries. Although barbarity and cruelty were common enough in the past, the twentieth century has witnessed human bestiality in a manifestation and scope heretofore unknown. The sheer number of the victims of Nazism and communism worldwide can be only estimated at 100 million people. The legacy remains horrifying, as recorded and revealed in the vast literature already available. In *The War of the Doomed*, Shmuel Krakowski describes the conflict in Poland between effective killers and those who were determined to resist evil even at the cost of their lives. He concentrates on the Jewish armed resistance to Nazi terror during World War II in the General Government (GG), located in central Poland.

The substance of the book can be better understood when one considers the number of Jews who perished. There were 3.25 million Jews in Poland in 1939; less than 10 percent survived to 1945. The largest number were exterminated in various concentration camps of which Treblinka, Sobibor, Belzen, and Auschwitz have become the symbols of the Holocaust. Some ten thousand Jews, however, took up arms to fight the Germans either in the Warsaw Ghetto uprising of April 1943 or in numerous Jewish partisan units where most of them perished. Only several hundred survived as living witnesses of the Jewish contribution to the war against Germany.

It is this aspect of Jewish resistance that Krakowski studies, and he provides details and information previously unknown even to most experts. The Warsaw Ghetto uprising was not the only example of the Jewish struggle, as is generally known. The Jewish partisan movement in the GG predated Polish underground activities. It began in the summer of 1942, as a result of the German action to liquidate the ghettos by transporting Jews to the extermination camps. Escapees from the ghettos, labor camps, concentration camps, and prisoner-of-war camps became the first partisans, a year before the Polish units of the Home Army (AK), People's Army (AL), People's Guard (GL), and the Peasants' Battalions began to populate the forests and openly resist German occupation.

These Jews reached the protective forests often poorly armed—and even without arms—facing, on the whole, hostile Polish peasants and numerous obstacles to sheer survival. Securing weapons and food as well as protecting other refugees, women, and children made the Jewish struggle different from other partisan movements. Except for the

socialist and communist units of the AL and GL, the Jewish partisans faced not only their German enemies but also frequently suffered casualties from the largest Polish resistance movement (the AK), the official army of the Polish Government-in-exile. As in Yugoslavia, an external war against Germany existed in Poland as well as an internal war, waged by the AK against the AL and the GL, in which the Jews were identified as an enemy or only tolerated as a partner of the left-wing forces.

The book offers impressive reports on the activities of the Jewish partisans in the five districts of the GG, the Warsaw Ghetto uprising, and other ghettos and camps, concluding with the Jewish participation in the Polish Warsaw uprising of 1944. In the end, the most successful, yet most tragic, Jewish partisan activities took place in the forests of Parczew, Lubartow, Pulawy (Lublin district), and Swietokrzyska puszcza (Radom district).

This book, like many others on the Jewish fate in Europe, makes emotionally disturbing reading, especially the parts describing the lack of support, the prevailing anti-Semitism among non-Jews, and the cold-blooded killing by German soldiers of unarmed and helpless Jewish men, women, and children. The major drawback to this book is that Krakowski does not offer any explanation for the origins and causes of this anti-Semitism.

The book provides several maps to assist the reader in locating the places mentioned in the text, a list of abbreviations of the terms used, an extensive body of sources and literature, and a complete name index. The superb translation makes rewarding reading in spite of the nightmares Krakowski describes in detail.

STEPHAN M. HORAK
Eastern Illinois University

KARL NEHRING. *Adam Freiherrn zu Herbersteins Gesandtschaftsreise nach Konstantinopel: Ein Beitrag zum Frieden von Zsitvatorok (1606)*. (Südosteuropäische Arbeiten, number 78.) Munich: R. Oldenbourg. 1983. Pp. 231.

Karl Nehring's work on the Treaty of Zsitvatorok (1606) and the mission of Adam Freiherrn zu Herberstein, imperial representative to Constantinople, will be welcomed by diplomatic historians and specialists in the history of the Holy Roman Empire, the Ottoman empire, and Southeastern Europe. It illumines the tangled skein of international relations and the diplomatic maneuvering between the Turks and the Habsburgs in the early seventeenth century. The title is somewhat misleading because the bulk of this work consists of the journal of Maximilian Brandstetter, secretary to Herberstein, which describes such locales as Con-

stantinople, Sofia, and Belgrade and provides sidelights on the nature of Ottoman rule and religion. Nehring has annotated the journal and appended a workable bibliography and indexes.

In the introduction, written in eminently clear German, Nehring analyzes in detail the treaty and Herberstein's mission, gives valuable background on the Habsburgs and Hungary, and discusses the significance of events such as the death of Stefan Bocskai. The Treaty of Zsitvatorok, which concluded the so-called Long (or Fifteen Years') War, also ended Habsburg tribute to the Ottomans. For the Ottomans, the territorial gains were insignificant; the treaty was important because it enabled them to concentrate on the war with Persia, deal with the problem of the janissary revolt of 1603, and recover from the devastation of the war. For the Habsburgs, peace was essential for them, too, faced acute financial difficulties; they were unable even to provision and pay the troops on the borders. The Habsburgs also faced the stiffening resistance of the diet and other domestic problems, notably, religious dissension and political unrest that was to erupt subsequently into the Thirty Years' War. Rudolf II, unpopular and frequently incapable of ruling, desperately tried to retain his hold on the imperial throne, prevent his younger brother Matthias from ruling, and end the revolt among the Hungarians who had chosen Stefan Bocskai as their ruler. As might be expected, difficulties remained even after the peace, for the treaty failed to address such key questions as the governance of Transylvania. The different versions of the treaty also caused problems. It was to address such issues that Herberstein was sent to Constantinople. In particular, he was to negotiate the return of the key fortresses of Kanisza, Gran, and Erlau, obtain the release of prisoners of war, and resolve the ongoing border disputes.

This valuable work adds to the scant literature available on Southeastern Europe in the seventeenth century. Be warned, however, that this book is for the specialist, because it presupposes an extensive knowledge not only of the Habsburg and the Ottoman empires but also of Hungary, Moldavia, Wallachia, and Transylvania. Nonspecialists would be well advised to consult standard works such as Peter Sugar's *Southeastern Europe under Ottoman Rule, 1354-1804* before embarking on this volume.

MARSHA FREY
Kansas State University

DOMOKOS KOSÁRY. *Művelődés a XVIII. századi Magyarországon* [Culture in Eighteenth-Century Hungary]. 2d ed. Budapest: Akadémiai Kiadó. 1983. Pp. 757. 184 Ft.

This impressive monograph, written by one of Hungary's senior historians, offers a survey of cultural life in the Kingdom of Hungary between 1711 and 1790. These dates are significant: the account begins with developments following the Peace Treaty of Szatmár, which ended eight years of anti-Habsburg warfare under the leadership of Prince Ferenc Rákóczi. In 1790 the traditionalists, including most of the Hungarian nobility, dealt a blow to the modernization initiated by Joseph II. The pre-1765 chapters present a model for the Hungarian "late Baroque," and the much longer second part of the book enumerates the achievements of the remaining twenty-five years. Domokos Kosáry succeeds in making clear that during the closing decades of the eighteenth century Hungary took the initial, often reluctant, and often hesitant steps out of the state of intellectual backwardness imposed on her in part by the Ottoman occupation and the draining experience of near-constant warfare.

In gathering and evaluating all available information on the cultural and intellectual life of a country of some nine million inhabitants around the end of the century, Kosáry has not ignored economic, social, and political developments. The volume makes its most telling contribution, however, by being the best-organized and most cogent account to date of the nonmaterial culture of eighteenth-century Hungary. In addition to using published research from various disciplines, the author has unearthed a wealth of primary sources previously hidden from the Western scholar by obscurity owing to their language or location in provincial archives.

The approach of the monograph itself is innovative. In contrast to traditional Hungarian historiography, which presents the eighteenth century as a period of "decay," Kosáry is more inclined to agree with scholars who argue that the epoch was one of gathering strength and fermenting ideas, preparation for the surge of regeneration that took place during the nineteenth century. By his understanding portrayal of Joseph II's policies and by his acknowledgment of enlightened absolutism's positive effect on the development of "peripheral" regions, Kosáry rejects the ethnocentricity and isolationism so prevalent in the historiography of Central Europe. His observations are made even more convincing by well-chosen references to events in neighboring countries.

Against a background marked by the departure of Hungarian consciousness from an exclusively religious orientation, Kosáry presents a wide panorama of spontaneous and institutional factors aiding the limited, though not inconsiderable, achievements of the Hungarian Enlightenment. He discusses, among others topics, the Freemason lodges, the reading circles, the earliest proponents for the

establishment of a Hungarian Academy of Sciences, the periodical publications, as well as the scientific and technical innovators and their contributions to agriculture and mining. The chapters on education are particularly well documented and offer a survey of a topic that has hitherto been scarcely examined. In accordance with the author's well-known interest in and sensitivity toward the arts and literature, the book presents a rich overview of creativity in eighteenth-century Hungary and the lively and fruitful interaction between Hungarian men of letters and their contemporaries abroad.

This volume is among the first important publications in Hungary's promising process of "rehabilitating" the long-neglected discipline of cultural history. Kosáry's reluctance to become mired down in abstractions resulted in a book that, in spite of its length, remains lucid and exciting throughout. Its success is attested to by the fact that it was reissued within three years of its original publication. In questioning the validity of certain *Friihaufklärung* theories (especially that of Edward Winter, which limits the advent of early Enlightenment to the lands inhabited by Germans, Austrians, and Slavs, thus placing the Hungarian area beyond the periphery of this intellectual movement), Kosáry's work is not without controversial aspects. These, however, are of the kind from which scientific inquiry can only profit, because they encourage further research into a scarcely studied period in the history of a region that is less than fully understood.

ANDRAS BOROS-KAZAI
Indiana University

MÁRIA ORMOS. *Padovától Trianonig, 1918–1920* [From Padua to Trianon, 1918–1920]. Budapest: Kossuth. 1983. Pp. 451. 60 Ft.

The Paris peace treaty of 1947 between the victorious Allies and Hungary returned the latter to frontiers first defined by the Treaty of Trianon (1920). This reduced Hungary to one-third of its pre-World War I size and forced 2.5 million Hungarians, or 25 percent of the population, to live under the sovereignty of Hungary's neighbors. Interwar Hungarian historiography dwelt on the injustice of the dictated peace, thus providing ammunition for the integral revisionism of the Horthy regime. Today a revision of Trianon—and hence of the Paris treaty—is out of the question since the Soviet Union, a major force behind the 1947 treaty, dominates the region. Passion, therefore, can give way to careful academic examination of the policies leading to Trianon.

An example of the consequent scholarship is Mária Ormos's monograph, which examines the impact of French policy on Hungary and the other

successor states from 1918 to 1920. Based on documents from the archives of the French Ministries of War and Foreign Affairs, this book is an important companion volume to recent monographs dealing with French war aims against Germany and Bolshevik Russia.

Ormos identifies French goals in the closing days of World War I as being of Napoleonic proportions, aiming at political and economic domination of Central and Eastern Europe as well as of Russia. In 1918, she notes, France did not have the military wherewithal to attain these goals: French expectations were based on the weakness of the states in the region (p. 51). The subsequent failure of France's design to set a Rhine frontier with Germany and the collapse of intervention in Russia also affected French policy vis-à-vis Hungary and its neighbors. France was unable to shape a territorial settlement in the Danube basin that would have been less debilitating to Hungary and would have made this successor state as pro-French as the others.

The territorial squabbles between Hungary and its neighbors are seen as the result of exaggerated and conflicting nationalisms. No state wanted frontiers based on ethnic boundaries, and the Hungarians are faulted for failing to recognize that Hungary was no longer the dominant state in the area. The liberal democratic Károlyi regime (November 1918–March 1919) stood for Hungarian territorial integrity. The government was in reality open to compromise, but this willingness was not reflected in official pronouncements. The communist Kun regime (March–July 1919), which Georges Clemenceau saw as having risen out of national despair, also missed opportunities that could have resulted in some favorable modifications to the new frontiers.

Despite the severity of Trianon's terms, which derived in part from the failure of France's grand design, Ormos does not consider France the *bête noire* perceived by interwar historians. Without France, she claims, Czechoslovakia, Yugoslavia, and Romania would have gathered up even more ethnically Hungarian lands than were granted to them by the Treaty of Trianon.

Ormos has made good use of the French documents to buttress her thesis. Unfortunately, the archives of Hungary's socialist neighbors are still closed, even to a Marxist scholar. These documents could have helped Ormos define the foreign policies of neighboring countries more sharply. What is surprising, however, is that Ormos failed to consult various Hungarian archival sources, which could have modified some of her views. For example, it is imprecise to contend that the Apáthy-Berthelot agreement (December 31, 1918), which revised the demarcation lines in Transylvania, demonstrated Hungary's willingness to compromise (pp. 125–27). According to the minutes of the council of ministers,

the government disavowed the deal because it undermined the Belgrade Military Convention of November 1918, which offered a demarcation line more favorable to Hungary. Hungarian inclination to compromise, therefore, was limited to areas that were not covered by this convention.

Despite this oversight, Ormos's book is a major contribution. The English translation, which is scheduled to appear soon, is highly commended to the readers of this review.

PETER PASTOR
Monclair State College

ALICE TEICHOVA and P. L. COTTRELL, editors. *International Business and Central Europe, 1918–1939*. New York: St. Martin's or Leicester University Press, Leicester. 1983. Pp. xxvii, 459. \$37.50.

This impressive book is the outcome of a three-day conference held in 1979 at the University of East Anglia on the relationship between West European multinational enterprises and their subsidiaries in the successor states of the Austro-Hungarian empire. Sixty-six participants, well known in their respective fields of economic history, heard sixteen presentations and as many comments on marketing and investment strategies, entrepreneurial successes and failures, the relationship between financial and industrial firms, and the different attitudes of capitalists in the various West European nations to the penetration of the Danubian markets in the interwar period.

After the demise of the monarchy, West European businesses sought to move into the void left by the failure of German and Austrian corporations to recover their prewar hegemony in this part of the globe. One group of essays consists of case studies of direct foreign investment in the region. These include papers on the Swedish Match Company; the French investment bank, the Banque de l'Union parisienne; and the French holding company, the Union Européenne industrielle et financière. Particular attention is paid to the mechanism of capital flows and financial penetration by banks. Other contributions examine the attempts of German and Austrian industrial firms to maintain their footing in Eastern and Southeastern Europe in the face of the new competition. The detailed studies are on I. G. Farben, Mannesman, Siemens, Österreichisch-Alpine Montangesellschaft, and the Austrian Credit-Anstalt. In addition, one essay shows how Nazi Germany regained economic hegemony over its Eastern European satellites. The author of this essay, R. Overy, concludes that by 1941 Hermann Göring had created the largest economic enterprise in Europe (p. 279).

Yet another group of essays concentrates on exploring the industrial structures of these economies and the trend toward concentration in the region both before and after the First World War. The question of the relative benefits conferred on the participating countries by these multinational enterprises is one of the issues raised. The authors point out that the high tariff walls erected by the successor states actually encouraged foreign enterprises to establish new factories there, thus increasing Western penetration of their markets. No consensus emerges among the participants on the question of the ultimate advantages and disadvantages of foreign capital and know-how to the region as a whole or to the individual nations concerned.

Such a short review clearly cannot do justice to a volume that is so rich in detail and that raises so many fundamental issues. It is clear that these essays are solid contributions, indispensable to business historians, and of real value to those who are interested, even peripherally, in the economies of East-Central Europe during these troubled decades.

JOHN KOMLOS
University of North Carolina

NARCISA LENGEL-KRIZMAN. *Zagreb u NOB-u* [Zagreb and the National Liberation Struggle]. Zagreb: Globus. 1980. Pp. 297.

Although the dust jacket of this book bears the title *Borbeni Zagreb* (Fighting Zagreb), the title page indicates *Zagreb u NOB-u* (*Narodno oslobodilačkoj borbi*) [Zagreb in the National Liberation Struggle (NOB)]. The author, Narcisa Lengel-Krizman, specializes in the history of the communist-led activities and armed struggle in Croatia primarily during the crucial years 1939–45. She works in the well-known Institute for the History of the Workers' Movement in Croatia. Even though there is an extensive literature about the NOB, glorifying the people's struggle against wartime occupiers, the publisher points out in the introduction that the sources available to historians are "frequently meager and also contradictory" (p. 6). The author has 258 footnotes of which some are very extensive.

In the postwar Yugoslav kingdom (1918–41), Zagreb, the beautiful Croatian capital, became the main financial and industrial center with a large concentration of dissatisfied proletarians. It was also the center of Croatian national, cultural, religious, and political activities. After the assassination of the leader of the Croatian people and Croatian Peasant party, Stjepan Radić (1928), King Alexander introduced a harsh military dictatorship. Among thousands of persecuted, imprisoned, tortured, and liquidated persons were many nationalists and communists.

Glimpses of this turbulent history are given in the first chapter, "The Workers' Movement in Zagreb in the Interwar Period." The extensive second chapter describes "embattled Zagreb" in the Independent State of Croatia (1941–45) during the controversial Ustasha period. Some two hundred pages of the third chapter, "The Development and Activities of the National Liberation Movement in Zagreb, 1941–1945," present the gist of this monograph. Interesting to note is the fact that Tito and the leadership of the Communist party of Yugoslavia resided in Zagreb until late spring of 1941, when Tito moved to Belgrade to start (after June 1941) a communist-led uprising and guerrilla struggle against the Germans and other pro-Axis forces.

For several years during the war, the only link between Tito and the Comintern in Moscow was through a radio station in Zagreb managed by Josip Kopinič. On page 231 the author mentions him as a mere radio operator, but he was the Comintern's—that is, Stalin's—main representative in occupied Yugoslavia! His role, as well as that of the Croatian communist Andrija Hebrang, who was liquidated at the end of the 1940s, and the question of responsibility for the disastrous outbreak of jailed communists from the Kerestinec camp in the summer of 1941 are now discussed in the Yugoslav media, but the author avoided discussion of these topics in her book.

Trying hard to prove the existence of widespread resistance against the Croatian state and its supporters, she quotes hundreds of incidents and gives too many statistics. This makes parts of the book redundant and boring. Like many Marxist historians, she uses double standards. Violence and terror by the communists are praised as heroic deeds, but, when those same methods were used by anticommunists, she brands them as fascist terror.

Until his dismissal from the party, Franjo Tuđman was the director of the same institute where Lengel-Krizman now works. He wrote extensively and edited books dealing with the NOB. In her book she does not quote these sources, nor does she mention his role as a partisan general! Her conclusion is that resistance in Zagreb was a very important part of Tito's whole movement and that the Croatian communists did play a more important role than is generally admitted by many Yugoslav historians of that turbulent and tragic time.

GEORGE J. PRPIC
John Carroll University

JOHN F. CADZOW *et al.*, editors. *Transylvania: The Roots of Ethnic Conflict*. Kent, Ohio: Kent State University Press. 1983. Pp. vi, 368. \$32.50.

According to the editors of this long-awaited study, the confrontation between Romanian, Hungarian,

and German inhabitants of Transylvania is potentially "one of the most troublesome ethnic minority crises of the current age." Although it has existed at least since 1918 and has grown in intensity since the Prague invasion of 1968, this confrontation is not as well known in the West as are those in Northern Ireland, the Basque region of Spain, and Cyprus. To insist that the Transylvanian problem threatens the foundations of world order and the peace settlement of 1919 (as reaffirmed after World War II and at the Helsinki conference) may be overly dramatic. And yet this volume performs a service by showing the American public that Transylvania is an actual place inhabited by millions of real people with serious problems and is not the fog-enshrouded land of Bram Stoker's fiction and of late-night movies, an image that even scholars have admittedly helped to perpetuate.

John F. Cadzow *et al.* have published the essential papers presented at the Kent State University Symposium on Transylvania, held May 18–20, 1979, and have included one or two additional studies to enhance coherence and chronology. Some appear as originally presented, but many have been revised and expanded. The arrangement is for the most part logical, spanning the Transylvanian question in all its complexities from the multiethnic Hungarian kingdom of the late fifteenth century to Ceaușescu's policies in 1978 toward his nation's 2.4 million Hungarians and 350,000 Saxon-Germans.

Part 1, "The Dawn of National Consciousness," offers the valuable conclusion by Leslie Domonkos that there is nothing predetermined or historically inevitable about ethnic conflict or forced assimilationist policies, neither of which existed in late medieval times in Transylvania. Béla Király insists that in human, physical, and economic terms the land was always "an integral part of Hungary" (p. 83) and calls events of 1914–18 an accident, for no objective factor bound the province to Romania. Yet he stresses just as firmly that Transylvania, as a part of Western civilization, ought to be a bridge between Romania and Hungary, not a source of conflict.

In "Nationalism and the Polarization of National Destinies," Joseph Held, Paul Bödy, Istvan Deak, and others demonstrate how the basic patterns of coexistence between peoples in Transylvania came to be shattered by Habsburg centralization policies and the rise after 1789 of nationalist movements. Part 3 focuses on Transylvania as a factor of great power policies and international relations between 1913 and 1946. Peter Pastor describes the complex maneuvering in Budapest and Bucharest and the Western-imposed "non-solution" that "left a Magyar irredenta in Romania and a revisionist state in Hungary" (p. 176) in 1919. In an especially interesting analysis, Stephen Fischer-Galați argues that since World War I the issue of Transylvania "has

been primarily a direct or indirect function of the actual, potential, or perceived dangers posed by the Soviet Union and Soviet communism in Eastern Europe" (p. 180); in short, what happens in Transylvania is in large measure still determined by the Kremlin.

"Contemporary Romanian Policies in Transylvania," the concluding section, includes Andrew Ludanyi's cogent dissection of the Daco-Roman theory as put forth in current Romanian historiography, along with documented accounts of minority-rights violations by Bucharest in its educational, social, cultural, and legal policies in Transylvania. A twenty-six page chronology of Transylvanian history, four lengthy appendixes, and a very decent index complete this welcome and relevant volume.

GERALD J. BOBANGO
Romanian-American Heritage Center
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BARBARA JELAVICH. *Russia and the Formation of the Romanian National State, 1821–1878*. (Joint Committee on Eastern Europe Publication Series, number 13.) New York: Cambridge University Press. 1984. Pp. xii, 356. \$44.50.

Drawing on her own earlier publications and on significant new research, Barbara Jelavich has written a close-to-definitive history of Russian policy toward the Romanian principalities of Moldavia and Wallachia in the nineteenth century, that is, before the attainment of formal independence by Romania. Bilateral relations, of course, were but a fraction of the full story, for the fate of the principalities was an integral part of the Eastern Question involving the Ottoman empire and all the powers of Europe. One of the author's most difficult tasks was to tell enough, but not too much, of that more extensive story in order to provide the necessary context for understanding Russian policy in the principalities without getting lost in the complexities of a century of European diplomacy. She has found a proper balance. This is a well-defined and well-organized monograph that enlightens but does not unduly burden the reader, whether his main interest is Russian foreign policy, Romanian history, or the pattern of European diplomacy.

Jelavich was fortunate in having available the basic source material for both sides. Copies of documents from the Russian Foreign Ministry were accessible in Bucharest, as were the diplomatic and other records of the two principalities. She has also used other unpublished sources and consulted all the recent monographic studies by Russian or Romanian scholars. Up to the Treaty of Paris in 1856, she tends to follow and summarize existing accounts, making no striking original contribution of

her own. But, in describing the subsequent process by which the two principalities, wards first of Russia and then of Europe, became the united independent state of Romania, she combines newly revealed information and judicious interpretation to make this a book of special value to historians of the period.

The overall picture of Russian policy is hardly one of imaginative strategy and sterling performance. The Russians had many advantages—conferred by geography, military power, and their role as patron of Orthodox Christians—but their policies were plagued by ambivalence and handicapped by the disastrous results of the Crimean War—internal weakness and international isolation. Romanian nationalism could not be suppressed by the tsar's fiat, and other powers, notably Austria, would not give him a free hand in the principalities. In the end, despite a victorious war against the Turks that Romania was forced to join to avoid being overrun, Russia could not reclaim the dominant role it had won in the first half of the century in the principalities, which were vital to Russian ambitions in the Balkans.

As for the Romanians, their record was one of taking advantage of their own relative weakness, maneuvering among the great powers, and making daring use of the *fait accompli*. Subject to limitations on their sovereignty and to many other constraints, the United Principalities could not make decisions of war and peace, but they could and did practice the arts of national self-preservation with remarkable success.

There may be broader lessons in this detailed account, relevant both to the nineteenth century and to Romania's position today. With the disappearance of the Concert of Europe and the classical European balance of power in this century's wars and revolutions, Russia has regained and is striving to maintain the protectorate lost over a century ago. Meanwhile, the Romanians continue to rely on their wits and their sense of nationhood. Jelavich, characteristically, does not engage in this kind of speculation. Her conclusions are measured. She is too careful a historian to be caught out by a reviewer for going beyond the evidence.

JOHN C. CAMPBELL
Cohasset, Massachusetts

PROCOPIUS PAPAISTRATIS. *British Policy towards Greece during the Second World War, 1941–1944*. (International Studies.) New York: Cambridge University Press. Pp. vii, 274. \$49.50.

"Greek politics is a hot and sticky porridge and I abhor the idea of putting my fingers into this mess" (p. 23) was the view expressed in November 1941 by

the British wartime foreign minister, Anthony Eden. Not usually known for the *bon mot* or for policy consistency, Eden and the British government found themselves by war's end up to their necks in the so-called Greek "mess." Indeed, Anglo-Greek relations from 1941 to 1944, the subject of this book by Procopis Papastratis, developed into a diplomatic imbroglio of staggering proportions that was often on the verge of chaos and rarely off the cabinet agenda. The final outcome, in Papastratis's opinion, was, nonetheless, a remarkable degree of success.

The causes for the diplomatic imbroglio are examined in minute detail. The first cause was rooted in the interrelationship of three competing centers of power—the British and Greek authorities divided between London and Cairo, the Greek resistance movements, and the Axis occupation powers in Greece—which made decision making a virtual nightmare. The British intelligence services, SIS and SOE, squabbled with each other and with the Foreign Office; royalist Metaxist supporters clashed with republican Venizelist circles; and an ongoing civil war was fought in Greece between Colonel N. Zervas's National Republican Hellenic League (EDES) and the communist-dominated People's Liberation Army of Greece (ELAS). Second, British authorities faced a delicate conflict between short-term military objectives and long-term political ones. Eventually, this led to the biting accusation that the British intended to "reimpose the King at the point of British bayonets" (p. 148). Third, Greece's wartime leaders, from King George II and politicians such as E. Tsouderos to the military and the Resistance ("miserable Greek banditti" [p. 195] in Winston Churchill's opinion), proved to be second rate, thoroughly unpopular, and notoriously unreliable.

Out of such ingredients, the British government had to fashion a foreign policy, plan strategy in the eastern Mediterranean, maintain a consensus among exiled Greeks, and orchestrate the Greek Resistance. British objectives throughout remained clear in theory, if not always in practice. This amounted to retaining Greece as a British sphere of influence and guaranteeing a pro-British government after the liberation. The ultimate irony of this book is that Britain's success owed as much to Joseph Stalin's acquiescence in these objectives as to British diplomatic skills or military prowess.

Papastratis's study of Anglo-Greek wartime relations will satisfy even the most curious for a long time to come. He has made diligent use of the British Foreign Office archives and was hampered only by the unusually large number of files that remain closed for periods of up to fifty years. His use of Greek sources encompasses the Tsouderos diaries and select other materials in the Greek state

archives, interviews with several wartime Greek leaders, and existing Greek-language materials on the subject. When this book is placed beside the work of J. S. Kiliopoulos and D. Kitsikis on the 1935–41 period, C. M. Woodhouse's memoirs and analyses of the Greek resistance movement, and G. M. Alexander on the 1944–47 years, the subject of Anglo-Greek relations will have received fair and comprehensive treatment after years of neglect.

SIDNEY ASTER
University of Toronto

PAUL R. GREGORY. *Russian National Income, 1885–1913*. New York: Cambridge University Press. 1982. Pp. xiv, 359. \$29.95.

Paul R. Gregory's latest book delivers more than the title promises, since he is also concerned with comparing the economic growth of late tsarist Russia with that of the Soviet regime. The bulk of the text is indeed devoted to a discussion of the growth rates of Russian national income from 1885 to 1913. He concludes that the pattern of Russian development in the decades before the war and the revolution corresponded to one of "modern economic growth," by which he seems to mean it was similar to the pattern of other industrializing *capitalist* countries, except for a higher rate of government expenditures. He argues that the shift of the work force from agriculture to industry, growth in productivity, and the pattern of relative prices, investments, and gross national product also had the same shape as those of other countries.

Some of this is surprising, especially the indication that Russian agricultural productivity in general and the rates and amounts of farm consumption in kind in particular continuously increased (though not without frequent backsliding) not only after 1907 but in the 1880s and 1890s as well. Also surprising is his conclusion that real income in Soviet Russia in 1928 was lower than in tsarist Russia in 1913 by a factor of 15 to 20 percent.

Important to his conclusions is a comparison with the model provided by Alexander Gerschenkron, which Gregory finds confirmed in some parts and denied in others. Although the discussion is of great interest to historians of Russia because of Gerschenkron's stature, it is marred by Gregory's insistence that Gerschenkron's model of Russian development be labeled "Asian"; my own belief is that Gerschenkron's typology of industrialization patterns is much richer and more subtle than the use of that term would justify.

Gregory suggests that most readers will be interested in the first chapters and the last, since the middle ones and the appendixes are more technical in nature. Throughout the book he provides help-

ful, if rather repetitious, summaries of his conclusions. The reader would be well advised to struggle with the technical portions of the book to the extent that he or she can. I found it enlightening to realize once more how many figures and charts are based on by-guess-and-by-golly, however well informed and judicious the choice and manipulation may be.

We historians are too often statistical naifs, inclined to take figures as if they were the result of direct counting, something like what the decennial American census purports to be. Economic statistics are rarely the result of individual enumeration; bushels of wheat, tons of steel, money spent on hair-cuts and TV repairs are not, and cannot be, numbered and summed up one by one. The totals are usually arrived at by sampling and extrapolation; although they often have a high level of precision and certainty, they nonetheless are approximations. Gregory's appendixes and much of the discussion in the text show that generally accepted figures were drawn up from scratch and then enshrined, with little further reflection on how casually they were obtained. This book should renew discussion of them. Gregory indicates a program for further research and a healthy degree of skepticism about his own results. Some of them evoke more doubt in the reviewer than in the writer.

The book is of incomparable physical ugliness, a shame to the Cambridge University Press. Word-processed books may be the wave of the future, but they need not be hideous or nearly illegible (as in the crucial summary tables 3.1 and 3.2). Furthermore, word-processed productions still require editorial guidance and correctives, which seem to be lacking here.

DANIEL MULHOLLAND
Tufts University

TIMOTHY EDWARD O'CONNOR. *The Politics of Soviet Culture: Anatolii Lunacharskii*. (Studies in the Fine Arts, The Avant-Garde, number 42.) Ann Arbor, Mich.: UMI Research. 1983. Pp. xiv, 193. \$39.95.

This brief, scholarly monograph provides a useful biography of A. V. Lunacharskii, the first Soviet commissar of public enlightenment, in the context of the relationship between the Bolsheviks and the intelligentsia. Timothy Edward O'Connor has done research in the archives and libraries of Kiev, Moscow, and Leningrad and has used extensively the latest Western scholarship.

The author at the outset expresses his intention to discuss how Lunacharskii and the Bolsheviks dealt with inherited problems of the technical and cultural intelligentsia during a revolutionary period. The first chapter traces Lunacharskii's career before 1917, when he and his brother-in-law, Alexander

Bogdanov, created their "god-building" and "proletarian" theories of cultural transformation, in which socialism became a surrogate religion of collectivism. The second chapter considers Lunacharskii's role as a link between Lenin's new government and the intelligentsia during and immediately after the 1917 revolution. The three subsequent chapters examine his role in education, culture, and science.

O'Connor stresses Lunacharskii's role as a reconciler of two distinct groups in Russian society, namely, political revolutionaries and intellectuals. Yet these groups were often not distinct at all; Lunacharskii himself was both a revolutionary and intellectual. Furthermore, his vision of collective immortality, in which the individual lost his identity and achieved his salvation through the proletariat, was anathema to Lenin. The New Economic Policy was not so much a goal as a compromise for both men. O'Connor draws the dichotomy between politics and culture too sharply and does not explore the larger implications of revolutionary transformation.

This slim volume provides a useful synthesis of the works of Sheila Fitzpatrick, Kendall Bailes, and James McClelland. But its archival sources add little to our picture of Lunacharskii, and the book itself does not reflect a close reading of his writings. We still await a substantial political and intellectual biography of one of the most interesting personalities of the Russian Revolution.

ROBERT CHADWELL WILLIAMS
Washington University

BIANKA PIETROW. *Stalinismus, Sicherheit, Offensive: Das "Dritte Reich" in der Konzeption der sowjetischen Außenpolitik, 1933–1941*. (Kasseler Forschungen zur Zeitgeschichte, number 2.) Melsungen, Federal Republic of Germany: Schwartz. 1983. Pp. 456.

Traditional interpretations of Soviet foreign policy in the 1930s tend to stress its discontinuity: the ideological rigidity of the early Stalin years is seen as having given way to the greater flexibility of collective security, which in turn was upset by the Nazi-Soviet nonaggression treaty. Bianka Pietrow rejects this framework and offers an interpretation emphasizing the continuity of Stalin's foreign policy. This policy, she says, was rooted in Marxist-Leninist assumptions concerning the inevitability of a second "imperialist war," in an acute awareness of the expanding economic and military power of the Soviet Union, and in an iron determination to enhance Soviet security. Pietrow stresses that the Soviet Union was a revisionist power that had no stake in the preservation of the Versailles system. Stalin aimed to destroy this system and, with it, the "capitalist encirclement" that restrained Soviet influence beyond Soviet borders. "Socialism" was to be

brought to Eastern Europe, where new "peoples' democracies" would replace the conservative regimes licensed by the victors of the "first imperialist war."

Initially, these aims were veiled, and Stalin pursued a cautious policy designed to avoid Soviet isolation in an increasingly dangerous international environment. By early 1939, however, increasing Soviet economic and military strength gave him sufficient confidence to take the offensive. Stalin believed that Germany was far weaker than the Western powers and, thus, more likely to grant his demands. The conclusion of the Nazi-Soviet nonaggression treaty appeared to validate this thesis. According to most accounts of this treaty and its subsequent application, Hitler got the better of the bargain because he was able to conquer most of Europe and turn on the Soviet Union at a time of his choosing. Pietrow shows convincingly, I believe, that Stalin did not view his relationship with Germany in this way. Stalin believed that he was dealing with Hitler from a position of virtually unassailable strength. Thus, he was able to secure the favorable revision of the secret protocol of the Nazi-Soviet nonaggression treaty on several occasions and, in late 1940, was preparing to do so again. Stalin thought that Hitler could not do without his political and economic support in Germany's continuing war with Great Britain and that this dependence would increase with time. Refusing to believe the mounting evidence that Hitler was preparing to attack the Soviet Union, Stalin was genuinely surprised by Operation Barbarossa. Pietrow might have subtitled her book "Wishful Thinking in Moscow" or "The Limits of Marxist-Leninist Analysis."

The author has consulted an impressive array of sources. In addition to the voluminous historical literature on the subject, she has made good use of the German and British archives and all available Soviet documents. The latter are not easy to use, but through careful analysis Pietrow succeeds in extracting full value from them. The result is a stimulating and thought-provoking work that should be read by all specialists in the field.

RICHARD K. DEBO
Simon Fraser University

NEAR EAST

WILLIAM J. GRISWOLD. *The Great Anatolian Rebellion, 1000–1020/1591–1611*. (Islamkundliche Untersuchungen, number 83.) Berlin: Klaus Schwarz. 1983. Pp. xxiv, 333.

ADEL ALLOUCHE. *The Origins and Development of the Ottoman-Safavid Conflict (906–962/1500–1555)*.

(Islamkundliche Untersuchungen, number 91.) Berlin: Klaus Schwarz. 1983. Pp. viii, 202.

These two first books by Adel Allouche and William J. Griswold are both admirable studies of important topics in the social history of the Near East. The Safavid seizure of power in Iran and the official transformation of that land into the bastion of Shi'ism were events of profound social and political import. It is a merit of Allouche's dissertation that the international and social contexts of these central facts of Iran's religious history receive some emphasis. The narrower political focus in Griswold's narrative of the Anatolian rebellions at the end of the sixteenth century is softened by his asides on the climatic, economic, and Mediterranean background.

The rise of the Safavids was accomplished when a Muslim "mystic" order developed secular political goals and championed the lot of disgruntled pastoralists. Anatolian nomads fought for the Safavids and gave them the strength to impose a vision of religious purity on Iran. Safavid success, using Anatolian forces, gravely endangered Ottoman rule in Anatolia. The coronation of Shah Isma'il I in 1501 led to generations of brutal warfare between the Ottomans and the Safavids. While giving religious rivalries their due, Allouche weaves an attractive pattern of other controlling factors behind the warfare: threats to interregional trade, the role of the Safavids astride the Ottoman-Mamluk frontiers, outbreaks of tribal irredentism in Anatolia, and the comparative logistic and strategic skills of the enemies. All of these themes are outlined or alluded to on the basis of a variety of Arabic, Persian, and Ottoman sources.

The Ottoman struggle to control the Safavid threat, both within and without the Anatolian peninsula, strained the financial resources of the government and, in the countryside, the farmers' patience. The Jelali outbreaks, chronicled by Griswold, were but one response to the Ottoman heedlessness of the threats presented to agricultural prosperity by continual, distant campaigning. With the possible exception of the Janbulad (now called Jambulatt) emirs of Syria, none of the Jelalis sought independence. Higher status in the Ottoman ruling apparatus, a bigger slice of the revenue pie, freedom to handle local problems in their own way, and power over groups of musketmen—these were the comparatively low stakes for which the Jelalis fought. Yet just as the Safavid program represented a negation of the religious and political foundations of Ottoman power (and therefore the Safavids "deserved" extermination), so too, in a peculiar sense, did the Jelalis threaten the assumptions on which Ottoman expansion rested. Geography and technology had defined the limits of profitable Ottoman

power and centralized administration, and the Jelalis' proposed redistributions of authority would not only have brought unfamiliar faces to familiar positions but would also have hastened changes in the Ottomans' view of themselves and their mission. Griswold deftly mixes the evidence for Jelali conservatism with examples of the processes of change the rebels bequeathed to later generations. As does Allouche, he also demonstrates the benefits of examining sources from elsewhere in the Mediterranean area and gathers much important material for the first time from Italian collections. This book will be the standard handbook for future students of this troubled era.

Both books also point the way to various methods those future students may profitably adopt. The narrative sources and the Ottoman registers of orders sent out from Istanbul do not reveal much, if anything, about the particular causes of disaffection among particular groups in specific localities. A systematic examination of the tax records on agriculture, plus a close reading of local court records, may allow the next generation of scholars to describe the conditions that led to "apostasy" or banditry. For the student of technology, there is a striking contrast between the pastoral and nomadic cavalry raised by the Safavids (no match for the Janissaries and Ottoman ordnance) and the musketeers of the Jelalis (more than a match for the local Ottoman levies). In the course of the sixteenth century the Ottomans descended from a leading position in the application of new equipment and ideas to a lagging, even lazy, appreciation of even newer notions. This study will demand a mastery of both eras and will justify the comparative dimension that makes these two books together more exciting than they are separately.

These volumes and their authors exemplify a growing sophistication in the study of early Ottoman history. No longer is a source's bland ideological claim or categorization of an action to be accepted as fact rather than as artifact of certain (also interesting) intellectual currents. No longer are European social and religious categories applied directly to non-European behavior. Both these and other trends visible in the work of Allouche and Griswold are welcome.

RUDI PAUL LINDNER
University of Michigan

SURAIYA FAROQHI. *Towns and Townsmen of Ottoman Anatolia: Trade, Crafts, and Food Production in an Urban Setting, 1520–1650*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1984. Pp. xiv, 425. \$59.50.

This is primarily an economic history. Suraiya Faroqhi considers basic questions of production,

distribution, and consumption that have usually only been analyzed in articles, many of them in Turkish, on specific topics. As is obvious from the notes, bibliography, and scholarly content, Faroqhi's use of archival sources and secondary works is impressive. No other volume has brought together these materials in so complete and accessible a fashion. Faroqhi's study begins with a description of Ottoman Anatolian cities and their foundations. She extends her examination from the cities first to the trade routes that supplied the cities and then to specific aspects of agriculture, manufacture, and distribution. Despite the author's admirable efforts to present esoteric concepts and language in an understandable fashion, the volume will probably appeal mainly to Ottoman historians.

This study is strongest when it is most didactic, presenting and summarizing what is known about the economic life of sixteenth- and seventeenth-century Anatolia. One learns who the Ottoman miners were and what they mined, what textiles were produced and where and by whom. The author discusses the various Anatolian uses of honey, the practice of usury, the consumption of soap, and thousands of other basic aspects of economic life. These descriptions coalesce into a picture of Ottoman life in the period, particularly when read in conjunction with the earlier works of Halil Inalcik and others.

The book is weakest when it extrapolates from fragmentary data. The author frequently states that little research has been done on a topic and then proceeds to draw conclusions from those same deficient data. Many generalizations are drawn on the basis of evidence from only a few cities. Assumptions are often made without evidence. Although Faroqhi properly notifies the reader when such generalizations are being made, too much of the analysis depends on them. For example, in her chapter on migrants and urban development, Faroqhi assumes that there were significantly more deaths than births in Anatolian cities, because, she states, urban mortality was great elsewhere and "there is little reason to assume that Anatolian towns were any healthier to live in than their counterparts in seventeenth-century Europe, India, or China" (p. 267). Many, myself included, would contend that Anatolian cities may indeed have been considerably healthier than those in Europe. Anatolian cities in later periods did not show a net deficit of deaths over births, even though sanitation and medicine were no better than in the 1600s. The real difficulty, however, is that she concludes, on the basis of an unsupported assumption, that cities needed migrants to survive and that migrants were drawn to the opportunities provided by high urban mortality. Such analysis leads the reader away from explana-

tions of urbanization, such as rural overpopulation and rebellion, for which there is evidence.

In her preface, Faroqhi indicates that "the present study has been influenced by the massive regional monographs written by French historians during the last thirty years" (p. xii). She admits that the lack of sources makes such a study impossible for Anatolia, but she still feels the need to fill the gaps with assumptions. It might have been better to begin with a theoretical structure, perhaps an urban typology, even though the justification for the construct would have been incomplete, or to settle for a more purely descriptive work. Nevertheless, *Towns and Townsmen of Ottoman Anatolia* is a significant and valuable book, containing masterful summaries of aspects of Anatolian economic life.

JUSTIN MCCARTHY
University of Louisville

L. CARL BROWN. *International Politics and the Middle East: Old Rules, Dangerous Game*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1984. Pp. xii, 363. Cloth \$35.00, paper \$15.00.

L. Carl Brown has written an engaging interpretive essay that attempts to unravel the intricacies of international diplomacy in the Middle East. This is a formidable task, the more so since Brown attempts to accomplish it in less than three hundred pages. Moreover, whereas other scholars have tended to deal either with specific aspects of this complex and often emotion-laden subject or with clearly defined historical periods and episodes, Brown tackles the entire gamut of Middle Eastern diplomacy over the past two centuries.

Brown's thesis is that the international system of the Middle East has a distinctive culture and style that has prevailed since 1774 when the balance of power tilted permanently against the Ottoman empire. This development inaugurated what came to be known as the Eastern Question. Essentially, the "Eastern Question system" (Brown consistently uses the term as an adjective rather than a noun) revolves around the classic issue of nineteenth-century European diplomacy: how to dispose of the political and territorial legacy of the Ottoman empire. In his view, this issue has not yet been finally resolved.

Brown's interest in the topic was born out of his exposure to the "stirring events" of October 1973, which found him engaged in historical research in Cairo. His contribution inheres in his attempt to combine the expertise of a scholar steeped in the study of Middle Eastern culture and history with the facts of diplomatic history. He particularly faults diplomatic historians who have studied the Eastern Question from an excessively European perspective.

It is more appropriate, he argues, to regard the "Eastern Question system" as a "penetrated" political system, that is, "one that is neither effectively absorbed by the outside challenger nor later released from the outsider's smothering embrace" (p. 5).

Understanding the "Eastern Question system" thus requires familiarity with the rules of the Eastern Question diplomatic game. The essence of these rules involves kaleidoscopic interaction between indigenous actors and outsiders (that is, the great powers) in which the balance is not as heavily weighted in favor of the latter as is generally assumed. Brown emphatically rejects "puppet" theories of the past as well as the present, according to which the world's great powers are assumed fully to control the indigenous political actors. He singles out examples such as Lyndon Johnson, Elmo Zumwalt, and Lord Ponsonby (British ambassador to the Ottoman empire in 1836), each of whom was convinced that Russia was bent on complete domination of the Middle East.

In Brown's view, understanding the dynamics of the "Eastern Question game" enables us better to understand the political culture of a strange and confusing, as well as highly dangerous and volatile, part of the world. In particular, this approach calls attention to the stability of the rules of the game, which have remained essentially unchanged for more than two centuries, despite kaleidoscopic interactions of the players and sudden and dramatic changes in their identities and roles. Brown expresses the hope that his analysis will aid not only students of the Middle East but possibly policy makers as well.

Given this hope, it is somewhat disappointing to note that the only policy-related recommendation to emerge from this book is that the players of the Eastern Question game—both insiders and outsiders—should exercise prudent restraint in the pursuit of their political goals, because the game has become exceedingly dangerous. Unfortunately, Brown does not explain why such prudent restraint has been missing from the game all these years, why outsiders have been so persistent in meddling in the affairs of this hapless portion of our earth, or why, after more than two centuries, the perennial Eastern Question still awaits resolution.

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Chicago

CLIVE LEATHERDALE. *Britain and Saudi Arabia, 1925–1939: The Imperial Oasis*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1983. Pp. viii, 403. \$35.00.

This book provides a balanced, comprehensive, and

readable analysis of British policy toward Ibn Saud during the decade and a half before the outbreak of World War II. Ibn Saud had to make some concessions to the British in foreign relations, but, because Saudi oil had not yet become significant, he still maintained control over internal affairs. British officials thus secured their strategic interests and made Saudi Arabia a kind of "imperial oasis," but American oilmen gained a concession in 1933 that turned out to be an economic bonanza.

Clive Leatherdale avoids the hagiography of Ibn Saud that is characteristic of what has been called "the Philby-ARAMCO school of Arabian history." He also remains aloof from the ideological conflicts concerning imperialism and from partisanship in Arab and Palestinian affairs that has ensnared so many British in the Middle East. Instead, the book simply depicts the continuities of British strategic aims as well as all the interdepartmental squabbles. He also demonstrates how the shortage of personnel and the lack of reliable information about the peninsula led policy makers toward negative forms of pragmatism.

Separate chapters deal in detail with a variety of Anglo-Saudi issues. Northern frontier disputes between Ibn Saud and the British mandates in Transjordan and Iraq favored the British in the 1920s, and the British willingly conceded to Ibn Saud's ambitions in southeastern Arabia during the 1930s. Saudi-Yemeni disputes were eclipsed by Anglo-Italian rivalry in the Red Sea. The further Ibn Saud got from his own territorial interests, the more dependent he was on the British, as in issues like Saudi Arabia's entry into the League of Nations as well as pan-Arab and Palestinian aspirations. British appeasement of the Arabs on the eve of World War II affected Ibn Saud's regime, but appeasement was not caused by Saudi pressure or power.

To the British, Ibn Saud seemed less tractable than the Hashemites, the Trucial chiefs, and other Arab leaders who collaborated with them. The British recognized Ibn Saud's independence in the peninsula, which was consistent with earlier imperial practices against interfering with the holy cities of Islam. In Ibn Saud's relations with neighboring regimes, however, the British effectively kept the Saudi leader's ambitions in check so as to impose their own "Monroe Doctrine" in the Middle East.

Given British effectiveness, Leatherdale's treatment of Ibn Saud's historic Hasa concession in 1933 is important. At that time, the British had plenty of oil and did not contest granting an American oil company such a concession. The American offer was many times larger than the British one, and Ibn Saud's decision was apparently based on his need for revenue rather than opposition to the British.

ROGER ADELSON
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YEHOYADA HAIM. *Abandonment of Illusions: Zionist Political Attitudes toward Palestinian Arab Nationalism, 1936-1939*. (Westview Replica Edition.) Boulder, Colo.: Westview. 1983. Pp. ix, 173. \$17.00.

Turning a doctoral dissertation into a book is a difficult and tortuous undertaking. Inevitably, the worthy editor wants the lethargic prose cut, revised, and simplified. Lumbering and explanatory footnotes are pared as their numerical sequence changes. Personalities and events placed anew in the text demand additional amplification. Chapter headings are tightened, and a myriad of other changes take place. If there is time and persistence, the greatest obstacle is fending off those distractions and intermediate commitments that always seem to conspire against the author yearning to be published and seeking tenure or promotion.

Yehoyada Haim's book is an example of a 600-plus-page dissertation written in 1975 that eight years later has become a 173-page book. Its prose, footnotes, and content underwent the obligatory reworking. Like its mammoth predecessor, Haim's book adequately distinguishes the varieties of Zionist perceptions of the Palestinian Arabs in a critical period of the British mandate, 1936-39. The author demonstrates quite convincingly that Zionism was not politically monolithic or homogenous in content; even within its three major ideological components—"official Zionism," "revisionism," and "bi-nationalism"—there were shades of opinion, interpretation, and understanding on the issues of the day.

The key question for the Zionists was how to deal with the emerging yet fragmented Palestine Arab national movement. Some discounted its sincerity; others believed that the two nascent nationalisms would inevitably merge. Still others believed that the Palestinians could be coopted through economic, political, and socialistic means. The issues that Haim addresses were the critical ones for the Zionist political community—the legislative council notion, small-landowner legislation, degree of great power reliance, the timing and use of force, the Arab revolt, the Peel (Royal) Commission, partition, canonization, and British policies.

Where the book falters somewhat is in the overwhelming emphasis Haim places on Zionist political thinking about these events and issues, rather than on analysis of Zionist thought about Palestinian Arab nationalism as suggested by the book's subtitle. Haim tends to be almost totally descriptive. Dependent as he is on rich documentation from a variety of Zionist archives in Israel, he lets Zionist argumentation shape his conclusions. Haim only uses Zionist sources to portray British attitudes, omitting entirely the 733 record group on Palestine of the British Colonial Office.

Haim has made an important contribution, but *Abandonment of Illusions* retains vestigial traces of the dissertation: the absence of an insightful synthesis of his findings in a concluding chapter and the omission of excellent secondary material published in the period between the dissertation's writing and the book's publication. Notwithstanding these shortcomings, Haim has given us a limited but important glimpse into Zionist thinking, but he leaves the basic question unanswered: why were Zionists, with all their diversity and internal disagreement, able to prevail in the struggle for Palestine?

KENNETH W. STEIN
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JOAN PETERS. *From Time Immemorial: The Origins of the Arab-Jewish Conflict over Palestine*. New York: Harper and Row or Fitzhenry and Whiteside, Toronto. 1984. Pp. x, 601. \$24.95.

Joan Peters states that the goals of her research and her book are to refute Arab (and it is important to stress Arab here as Peters does not conceive of Palestine or Palestinian Arabs as having territorial or historical validity or meaning) claims that the Zionists (pre-1948) and Israelis confiscated Palestinian property and expelled Palestinians from their lands and their homeland in which they had lived "from time immemorial." Hence, the title of the book. Peters states that Jewish settlers in the late nineteenth century did not displace Arabs along the coast in Palestine where they settled, because this area was largely uninhabited. The sparse population that did exist was composed of various multiethnic peoples, many of whom were not Arab (p. 183), and included Arabs attracted to Jewish settlements from the western portions of the Palestine mandate as well as from outside this area. Thus, many of the denizens of the coastal region were not indigenous (p. 261). It follows from the above, in Peters's view, that in 1948 there were no Arab refugees, only "returnees," people returning to places from which they had emigrated ten or twenty years earlier. Hence, there is no Palestinian refugee problem. The Arabs have invented an identity for those refugees merely to counter Zionism. In doing so, they have engaged in a cynical rewriting of history (p. 171).

Peters attempts to substantiate further her claims that Jews did not displace an indigenous population by suggesting that there was a substantial immigration of 213,700 Arabs to Jewish settled areas in "Western Palestine." Thus, out of a total non-Jewish population that Peters calculates at 462,900, only 249,200 were "original" inhabitants. To Peters these data have a clear message: the Jews displaced few indigenous inhabitants and the Palestinians' claims to Israel on the basis of prior long-term settlement

are unfounded and untrue. Peters also suggests, although she provides no documentation, that there was fairly large-scale Arab immigration into Palestine. Furthermore, Britain followed a double standard regarding immigration to Palestine: it set limits and quotas for Jews but winked at Arab immigration. In fact, Peters contends that the British imported (p. 379) Arab laborers into Palestine during World War II who took jobs that Jewish concentration camp victims could have had. This action "may indeed have constituted active participation in the racial genocide of the Jews in Eastern Europe by Britain's officialdom in Western Palestine" (pp. 335, 382).

This is a startling and disturbing book. It is startling because, despite the author's professed ignorance of the historiography of the Arab-Israeli conflict and lack of knowledge of Middle Eastern history (pp. 221, 335) coupled with her limitation to sources largely in English (absolutely no Arab sources are used), she engages in the rewriting of history on the basis of little evidence. The only relatively new question (it has been raised before) and data that Peters brings to bear on the historiography of the Palestinian-Jewish conflict are those concerning the extent of Arab immigration into Palestine during the mandate period. As she concludes, the numbers here are uncertain. The undocumented numbers in her book in no way allow for the wild and exaggerated assertions that she makes or for her conclusion. This book is disturbing because it seems to have been written for purely polemical and political reasons: to prove that Jordan is the Palestinian state. This argument, long current among revisionist Zionists, has regained popularity in Israel and among Jews since the Likud party came to power in Israel in 1977. Its current champion in Israel is Arik Sharon. This book is disheartening to all people who truly desire some kind of equitable resolution to the Jewish-Palestinian conflict. It is also disheartening that so many scholars, such as Elie Kedourie and Bernard Lewis, so many prominent personalities, such as Saul Bellow and Elie Wiesel, and even historians, such as Lucy Dawidowicz and Barbara Tuchman, have endorsed, praised, and, in the case of Kedourie and Lewis, apparently aided in the publication of this undeserving book.

ROBERT OLSON
University of Kentucky

AFRICA

ABOUL KACEM SAADALLAH. *Tārīkh al-Jazā'ir al-Thaqāfi: Min al-Qarn al-Āshir ilā l-Rābi'* 'Ashara l-Hijri (16–20M.) [A Cultural History of Algeria: From the Tenth to the Fourteenth Century after the Hijra

(16th–20th Century A.D.)]. In two volumes. Algiers: Ash-Shirkat al-Wataniyat lin-Nashr wat-Tawzi'. 1981. Pp. 598; 518.

National histories have acquired a firm position in historiography. The evolution of Western nations since the late Middle Ages seems to fit well with the intuitively convincing biological growth-and-differentiation model. The model may be more applicable to some nations than to others, but, since at least some forms of growth can be discovered in all of Europe, the growth-and-differentiation model seems the most natural form for describing national evolution. By contrast, scholars dealing with the Middle East and North Africa have great difficulties with the model. Either growth and differentiation is hidden and must be discovered by hook or by crook or periods of stagnation must be admitted, in which case the model becomes messy.

About Kacem Saadallah opts for stagnation (*jumūd*) but struggles valiantly to keep the damage to the national growth-and-differentiation model to a minimum. He places himself firmly in the camp of Algerian national historiography by offering his work as a contribution to the recovery of the national heritage, damaged by French colonialism. A good deal of the cultural heritage of the nation stems from the Ottoman period in North Africa (1516–1830 in Algeria), on which the two volumes focus. This period had its dark as well as brilliant sides, so Saadallah asserts, and, even though he condemns the Ottomans for their "anarchy, disorder, corruption, violence and immorality" (p. 10), he does not attribute the entirety of Algeria's stagnation to them. In fact, the author goes to great lengths to differentiate and weigh a variety of arguments. The enormous amount of carefully balanced detail spread out in these twelve chapters causes one almost to forget that the book is not a detached scholarly work without declared political objectives but a national history intended to contribute to the building of the Algerian nation.

The book contains a broad and comprehensive intellectual history. It discusses culture from the bottom up: after an introductory chapter on the fifteenth century and another on general "currents and influences," Saadallah surveys "cultural foundations" and "instruction and personnel." In the remaining chapters he presents an overview of religious scholars, saintly personages, law, theology, language, poetry, history, and sciences. The author treats the three centuries of Ottoman rule as a single unit, avoiding any overall schematization of cultural evolution. Thus, although he seeks to reconstitute the Algerian past, each area of culture is examined on its own merits.

The stagnation so jarring for the writing of national history is kept as vague as possible in its

contours. Ibn Khaldun (d. 1406) and Ibn al-Azraq (d. 1491) are cited as early witnesses, and the wars of the sixteenth century between Spain and the Ottomans in Algeria appear as culminating events. Typical symptoms of cultural stagnation were excessive mysticism and fear of literary innovation. According to Saadallah, the stagnation was halted in the seventeenth century, and by the early nineteenth century a full revival (*nuhūd*) was in evidence. Unfortunately, the revival was short-lived because of French colonialism. Thus, in the nick of time, indigenous progress (*numūw*) reappeared and would have resulted in the establishment of a national culture a century and a half earlier, had it not been for colonialism. Whether cultural activities in Algeria during the eighteenth and nineteenth centuries were indeed of the proportions of a revival is debatable, but the author has to be warmly commended for his honest debate of the national issues at stake in the writing of a cultural history of Algeria, quite apart from the impressive material results of his research.

PETER VON SIVERS
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GERMAIN AYACHE. *Les origines de la guerre du Rif*. (Publications de la Sorbonne, série internationale, number 19.) Paris: Publications de la Sorbonne et Société Marocaine de Éditeurs Réunis, Rabat, Morocco. 1981. Pp. 374. 90 fr.

Germain Ayache, well known to students of Moroccan and North African history, has attempted in this book to explain a war that was forgotten by those who waged it. At the time the Rif War stirred writers and protestors, but it was soon over and, given the precarious condition of Europe in the two decades following the Great War, was overshadowed by other momentous events. The author points out that there are several ways of viewing the Rif controversy. It can be seen either as a struggle by humble peasants to fend off the armies of two great European powers or as a result of an inherent weakness in the ability of both France and Spain to administer the regions gained by the various treaties of the early twentieth century. Ayache holds closely to the second position. In doing so, he discusses in great detail the historic origins of the war in the Rif, and therein rests the value of the work. *Les origines de la guerre du Rif* examines, better than any other work on the subject, the historic antecedents that led to the outbreak of fighting in that area of Morocco.

Ayache examined a vast number of archival depositories in Morocco and in Europe while preparing this study. His bibliography reveals extensive research in Arabic sources in Morocco. Adding strength to his traditional labors is an impressive list

of oral sources, such as interviews with Moroccans involved with the Rif War. Ayache warns us that a half-century has passed since the event and "direct oral sources cannot therefore be very numerous." But it is to Ayache's credit that he preserved these sources by interviews, which were conducted in the early 1970s. Many of those persons contacted were close to Abd al-Krim. The use of this historical method gives the work a decidedly North African flavor with insights into the non-European aspects of the situation in the Rif in the 1920s.

Jacques Berque, in his *French North Africa: The Maghrib between Two World Wars* (1967), has written that "the rising of Abd al-Krim relegated [the Lyautey method of colonial control] into anachronism" (p. 77). Certainly, the Rif War was a great event in the Maghrib, and North Africans residing in France and participating in the 1924 North African Congress expressed sympathy and support for Abd al-Krim in the Rif. What Ayache also tells us is that there were cracks in the French Maghribi empire that no amount of imperialist propaganda from either Fez or Paris could conceal.

Les origines de la guerre du Rif, therefore, makes a definite contribution to several areas of history. First, Ayache's work deals with a period in colonial history often neglected. Post-World War I imperialism was overshadowed by European events, but Ayache is able to focus on those overseas events. Second, this work gives insight into North African society. Through the use of Arabic and oral sources, we have a better view of Maghribis and events shaped by the colonized. Ayache's work, well written and well documented, is a welcome addition to the growing body of literature in the field.

JAMES J. COOKE
University of Mississippi

MICHAEL WATTS. *Silent Violence: Food, Famine, and Peasantry in Northern Nigeria*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xxxi, 687. \$34.50.

Michael Watts has written a large, imposing volume that studies the relationships among agricultural production, climate, capitalism, the state, and famine in northern Nigeria from the early 1800s to the present. That region—independent during the nineteenth century, well known for the "indirect rule" of the colonial period, and victim of an unforgiving climate—is an apt setting for such a study.

Watts's effort should be well received by scholars because of the breadth and depth of his archival research. A reader who wishes to find a detailed account of long-term changes in the region's food and cash crop production will find it here. The evidence marshaled in his discussion of famine in

that area during the colonial and postcolonial periods is buttressed by field work, making it the most extensive treatment available. These are major attributes.

Watts makes a number of controversial value judgments. He argues that a precolonial "moral economy" based on reciprocity, mutual obligation, and pooling and sharing was stripped away by colonialism and exposure to the world market for export produce. High colonial taxes required cash for payment; cash required a shift to cash crops, primarily groundnuts but also cotton; the buffering effect of food-based agriculture was seriously weakened; the economy's vulnerability to a capricious climate and to famine increased. There was an "entrapment of peasants into a global net" (p. 252). "Crudely put, to starve to death the rural producers who were the material foundation of the colonial state was not an especially astute long-term exploitative strategy" (p. 273).

To have doubts about this chain of logic is not to carry a brief for colonialism or the northern Nigerian colonial administration. Watts does discuss in detail the economic mistakes of that administration, which put obstacles in the way of indigenous traders and advocated cotton when groundnuts were a much more profitable alternative. It also pushed taxes to a relatively high level for a society so poor, taxing especially heavily the producers of export crops. The administration often failed to realize the degree to which the conservative behavior of farmers was rational risk aversion and the extent to which their "primitive" agronomic techniques were suited to climatic risk. It failed to take significant steps to correct the maldistribution of income that worldwide is a key to understanding the severity of famines. Finally, its famine-relief schemes during droughts were paltry. All this is true. But Watts goes considerably further. He argues that as regards farming and famine the colonial period was a retrogression from the precolonial period. Such a case is, I suggest, questionable for several reasons.

First, the claim that the twentieth-century droughts have been more damaging in human terms than similar precolonial droughts must be treated with care. Economic historians have given more support than Watts does to the idea that income growth that allows for purchases of food and better transport is a powerful anti-famine tool and could well have outweighed any adverse developments in food production. Given the deficiency of data on rainfall, production, and mortality in the region before the arrival of the Europeans, it is not possible to be conclusive.

Second, the charge that colonial taxes were high in comparison to those of the precolonial period is surely mitigated by the phasing out of slavery. The region had a remarkably high slave population,

which we can assume bore the burden of periodic droughts and famines to a greater degree, surely, than did their masters. One can think of slavery itself, as well as endemic slave-raiding, as an implicit system of taxing the fruits of labor. For the large proportion of the population made up of freed slaves, the colonial cash taxes must have seemed a fine exchange for freedom from bondage. Even if slaves had been employed in producing a grain reserve for famine security, in a social sense these precolonial gains could hardly have equaled their costs.

Third, we must view skeptically the claim that linkage to the rigors and risks of the world economy and the diversion of food production into cash crops for export caused by the colonial authorities' tax policies were detrimental for farmers. What if colonial taxes had been zero, with public services provided gratis by a benevolent metropole? What would farmers have done in this circumstance? Would they have avoided the link with the world market and the clutches of the European firms and middlemen by continuing to grow only food and materials for household use? Surely not. Farmers would have taken en masse to groundnuts anyway, because this was an area of very low income and the cash could be used to satisfy wants.

Even with the colonial taxes, why did farmers not avoid the world markets and sell food that not only could pay the tax but also would lessen the risk of famine and be lucrative when crops were reduced during a drought? The answer seems clear. Farmers found the returns on groundnuts much higher. In normal years overproduction of food (the demand for which is relatively inelastic) would cause a break in prices—indeed, this has occurred more than once in northern Nigeria following famine years. Storage of food in a capital-short economy marked by high interest rates was (and is) expensive, so that addition to stocks was not a preferred route. Farmers chose an exportable cash crop because it paid them to do so. There was no compulsion beyond the tax bill, and, even had there been no taxes, farmers would surely have made the same production choice voluntarily.

Let us carry the counterfactual one step further: if there had been no colonization at all in northern Nigeria, would not a railway and road system eventually have been built by an independent noncolonial state? Would not groundnuts have yielded better returns than food, just as they actually did in the colonial period? Would not farmers have embraced "commoditization," whatever the attendant disadvantages, just as eagerly? If the answers are "yes" or even "maybe," as I think probable, then Watts's model of a cash crop economy that resulted from colonial pressures and worsened the general welfare is pushed too far.

Questioning the thrust of the theory is not the same as questioning the value of Watts's large research effort and the resulting highly useful compendium of information. And even the skeptic will find the analysis thoroughly thought-provoking. The book is a must for anyone doing further research in the subject.

JAN HOGENDORN
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HARRY A. GAILEY. *Clifford: Imperial Proconsul*. London: Rex Collings; distributed by Robert E. Krieger, Malabar, Fla. 1982. Pp. 209. \$22.50.

Having already written extensively on Sir Frederick Lugard and Sir Donald Cameron, Harry A. Gailey naturally turned to Sir Hugh Clifford, the one remaining important West African governor of the pre-World War II period not yet possessing a biography. Several circumstances favored the undertaking: first, Clifford left a full set of private and semiofficial correspondence; second, his career—beginning in Malaya, then taking him to Trinidad, Ceylon, the Gold Coast, and Nigeria, and ending in Malaya where he began—is varied, interesting, and significant; and third, Clifford was an unusual man, a prolific and successful novelist. The study is based primarily on the Clifford papers and supplemented with what appears to be uneven and fairly thin use of Colonial Office materials (the citations are considerably more extensive on West Africa, especially on Nigeria, than elsewhere). Gailey has compressed the public career, fictional output, and the comparatively little that can be said about the private man into a text of less than two hundred pages. This is a short life with little about the times.

With fluent dexterity, if not with much penetration or profundity, Gailey moves rapidly through the highlights of Clifford's career. Discussing such topics as mental illness, Gailey is fairly successful in re-creating Clifford the man. I admire Gailey's attempt to assess Clifford's ability as a writer, which he does primarily by comparing him with his friend and contemporary Joseph Conrad. I found no important errors. The well-known events are there: Clifford's tutelage under Sir Frank Swettenham in Malaya, his celebrated deprecation of the claims of the National Congress of British West Africa to represent millions of Africans in the interior, his rebuff of Lord Leverhulm's attempt to establish the plantation system in Nigeria's palm oil industry. Gailey is well known for his work on Nigeria, and he goes into considerable detail about the Lugard era before Clifford arrived. Otherwise, Gailey's treatment of Clifford's successive administrations is superficial and just barely enables us to see the man in context. The use of official sources has been limited.

Gailey has not attempted to use Clifford's career as a means of contributing substantially to the study of colonial societies, and the student of comparative colonial administration therefore learns disappointingly little.

This criticism is perhaps unfair since it really calls for an entirely different sort of book: not only the massive two-volume works of Margery Perham on Lugard and Keith Hancock on Smuts come to mind but also Roger Joyce's biography of Sir William MacGregor and John Flint's of Sir George Goldie. A certain amount of superficiality is inevitable in such a short biography, and Sir Hugh Clifford deserves a long one.

JOHN W. CELL
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FATIMA BABIKER MAHMOUD. *The Sudanese Bourgeoisie: Vanguard of Development?* London: Zed or Khartoum University Press, Khartoum; distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. x, 170. Cloth \$26.25, paper \$10.25.

According to Fatima Babiker Mahmoud, "the events of July 1971, when the local bourgeoisie conspired with the imperialist bourgeoisie to bring down the regime, underlined the necessity for a comprehensive study of the Sudanese capitalist class" (p. 6). This book is such a study. But, stripped of theoretical overview and muddled historical narrative, the book reduces to a thin, interesting summary of the personal lives of one hundred rich Sudanese businessmen.

The first three chapters of the book are largely introductory. The "historical background" of the nineteenth century and the Condominium is retailed simplistically and erroneously. For example, the use of English in southern schools is said to have "resulted in a prolonged conflict in the southern region" (p. 27), and "the value of the Sudan's foreign trade was continuously on the increase" (p. 34). Misspellings and both factual and typographical mistakes abound. The index of one and one-half pages lists among its few entries "Ashiqqa sect" and "Mahdist (pro-Ansar)." The bibliography is weighted toward books unrelated to the Sudan. Appendixes provide unwanted tables of disconnected statistics (like "Exports of Sesame as Percentage of Production, 1954-67" and "Frozen Meat Exports, 1969-73").

In chapter 4 the author begins to present original material, but errors diminish its reliability. One subject of a case study is said to have arrived at Port Sudan in 1892, that is, about fourteen years before the city was founded (p. 75). The "role of the bourgeoisie" in the Sudan's political history, the subject of chapter 7, is flawed. The author discerns,

for example, "the growing political importance of the educated Sudanese" after 1924 as "the anti-colonial movement continued to expand" (p. 135). It did not.

Chapters 4, 5, and 6 nonetheless make entertaining reading. The very rich, in the Sudan as elsewhere, are different, and this the author illustrates by referring to the life-styles of her one hundred subjects. Statistics concerning their households, possessions, tastes, consumption, children's education, and so forth are abundant proof of the level of prosperity attained by the country's economic elite. But ninety-six of the one hundred believed in the evil eye and all of them in omens (p. 119).

The author's empirical findings are certainly intriguing, but they are ironically diluted by the theoretical discussion and undigested historical gleanings that extend the book to 152 pages. Those findings themselves would make a useful study but were perhaps thought somehow deficient as a thesis.

M. W. DALY

University of Khartoum

ARTHUR KEPPEL-JONES. *Rhodes and Rhodesia: The White Conquest of Zimbabwe, 1884-1902*. Buffalo: McGill-Queen's University Press. 1983. Pp. xiv, 674. \$60.00.

This long, complex, beautiful book left the reviewer with mixed, although generally positive, reactions. Arthur Keppel-Jones is one of the most distinguished South African historians, and the book is obviously the culmination of a lifetime of research and thought. In many ways Keppel-Jones succeeds too well in his major objective, which was to explore his subject and time period so thoroughly that little is left unmentioned. The result is definitely not a lightweight work to be read for pure enjoyment or one that is scanned for generalizations concerning Victorian imperialism and the nature and peccadilloes of empire builders such as Cecil Rhodes, Sir Leander Starr Jameson, Alfred Beit, Charles Rudd, or J. Rochfort Maguire. It is, rather, a book to be studied, a reference work of great merit whose author has searched, over many years, the most important archives as well as the most obscure repositories in England and South Africa for his information. Keppel-Jones's prose is not inspiring but is matter of fact; he obviously had to sacrifice style in order to state as clearly as possible the complex machinations that attended the birth of the Chartered Company and the subsequent development of Rhodesia.

The focus of the book is on the period before the Boer War. The author devotes 588 pages to this era and only one short closing chapter to the effects of the war on the company and Rhodesia. Generally well-known themes are expanded on, such as Lo

Bengula's ambivalent position at Bulawayo, the Rudd Concession, the charter, Portuguese claims, the Pioneer Column, and the rebellions. To all these subjects few generalizations are added that are not already known by specialists. The strength of the book is in the many details provided by Keppel-Jones on each of these significant events. It is difficult to imagine that one could frame a question concerning developments in Rhodesia over this eighteen-year period that has not been asked and answered by the author, who in some cases provides the most esoteric type of factual material. These detail the intricacies of the many and complex dealings between various concessionaire seekers (such as E. A. Maund, Lord Gifford, George Cawston, and E. A. Renny-Tailyour) and also between allies of Rhodes (Rudd, Maguire, and "Matabele" Thompson). The author attempts to describe not only various actions, but also the motivations for them. This type of analysis is also extended to the major central government figures such as Marquis of Salisbury, Lord Knutsford, and Joseph Chamberlain.

The most interesting sections of the book are the two chapters where Keppel-Jones deserts his strictly political, diplomatic, and economic analysis to concentrate on the sociological. The first is a long chapter entitled "The White Society" where the reader is given the flavor of the way the masters of Rhodesia behaved and thought during the early years of their hegemony. This is followed by an even more informative chapter, "The Black Experience," where Shona and Ndebele society is examined. These two chapters introduce one of the major subject areas of the book—the rebellions in Rhodesia that followed quickly on the Jameson Raid and threatened Rhodes's control of his empire beyond the Limpopo.

Keppel-Jones's scholarship is impeccable. He did not really need the disclaimer in his preface concerning his use of secondary sources and his decision not to use the more general works of some authors. It is very difficult to reconstruct a complex series of events from only the raw materials of letters, memoranda, reports, and the account sheets that one discovers in the archives. Memoirs, biographies, and accounts written by participants after the fact, give, at worst, the flavor of the prejudices of the participants; at best they provide a glimpse of the way people thought and acted during a given epoch. Keppel-Jones has done an excellent job not only in searching out archival materials but also in balancing these against those secondary accounts he has chosen to use. There are enough photographs, all from the National Archives of Zimbabwe, to provide the reader with a clear picture of some of the people and places referred to in the text. The decision to use an end map of southern Africa was a wise

choice. A minor fault, however, is the relative lack of more detailed maps in the text; there are only two but should have been many more to act as guides, particularly for those readers not familiar with the geography of Zimbabwe.

There will probably never be a definitive biography of Cecil Rhodes, the enigmatic personality who was the driving force behind most of the important events contributory to the creation of Rhodesia. It is, however, difficult to imagine that anyone could add appreciably to this thoroughly researched, long, detailed story of how the various forces in England and South Africa were harnessed by Rhodes and his associates to carry out his vision of English primacy north of the Limpopo.

HARRY A. GAILEY
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PETER WARWICK. *Black People and the South African War, 1899–1902*. (African Studies Series, number 40.) New York: Cambridge University Press. 1983. Pp. xiv, 226. \$49.50.

This book is a commendable effort that adds new dimensions to our understanding of the Boer War. It destroys the myth of an exclusively white man's war and also sets the struggle "in the context of a complex and rapidly changing colonial society increasingly shaped, but not yet transformed, by mining capital" (p. xi). Peter Warwick has combed the English-language sources: official archives in Britain and South Africa, missionary correspondence, newspapers (including English text in black papers), and significant sets of private papers. Diligent as his effort was, however, his work shows the problems of reconstructing the role and fate of a largely inarticulate underclass.

Major chapters of the book deal with political and military events involving Africans in several theaters: the siege of Mafeking, defense of Bechuanaland, and Kgatla intrusions into the western Transvaal; rival intrigues in Basutoland; anti-Boer activity by elements of the Zulu, partly organized by Dinizulu; a successful revolt by Sikhukhune Pedi; Swazi neutrality; and the use of black and colored levies to defend the Transkei and western Cape. Although necessary for a rounded picture of black involvement, these chapters may prove less interesting to readers than those dealing with African assistance to both armies, with the recruitment, pay, and conditions of African laborers supporting the war effort, with the sufferings of the 115,700 blacks swept into sixty refugee (concentration) camps, and with the disillusionment of Africans in the aftermath of the war.

Boer commandos used Africans as laborers, *agterryers*, messengers, spies, and unarmed scouts;

British forces used them as laborers, transport riders (up to 14,000), messengers, spies, armed scouts, armed blockhouse sentries, assistants in denuding the land of sheep, cattle, and crops, and as local levies armed to deny the guerrilla forces the use of significant regions. Armed blacks aiding the British numbered from 10,000 to 30,000—laborers and others over 100,000.

Warwick shows that Africans often took over deserted or British-controlled Boer farms and benefited from the sale of the crops, the provision of transport, and from plentiful work and good wages. Thus, during the war, several "props of Colonial society" were loosened. They were, however, rapidly restored by the victors during reconstruction. Black hopes for extension of moderate Cape racial policies to the north were shattered by the terms of the peace treaty. African wages on the Rand were depressed and discipline tightened. Many black peasant cultivators suffered from the scorched-earth policy and got much less help than whites toward recovery, being thus forced into wage labor. In areas not devastated, however, an African peasant class gained strength from the economic opportunities of the war years.

Although emphasizing social history and using British sources, Warwick reveals very little about the attitudes of British officers and officials toward nonwhites. He also seems a bit obsessed with the mining industry (rather than power-political purposes of empire) as the cause and intended beneficiary of Britain's war effort. Consequently, he fails to address adequately that most important question: why Britain turned her back on blacks at the end of the war.

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ASIA AND THE EAST

RANAJIT GUHA. *Elementary Aspects of Peasant Insurgency in Colonial India*. New York: Oxford University Press. 1983. Pp. viii, 361. \$21.95.

This book offers a full exposition of the theoretical framework underlying the Subaltern Studies Series of writings on South Asia. Common to this work and the two volumes of that series Guha has edited is an interest in people—peasants, workers, and others of the lower classes—who possess a "subalternity" defined not purely by their class but also by their subordinate positions in a particular society.

Social historians will find much that is familiar in Guha's approach, but they will also encounter some divergent concerns. For him, peasants (subalterns)

are not only makers of their own destiny but also possessors of a keen political consciousness. "Hence the word 'insurgency' has been used . . . as the name of that consciousness which informs the activity of the rural masses known as jacquerie, revolt, uprising, etc. or to use their Indian designation—dhing, bidroha, ulgulan, hool, *fituri* and so on" (p. 4).

Guha's "insurgent" peasants emerge from an impressive analysis of a wide range of data relating to actions and beliefs. He principally covers the Rangpur *dhing* of 1783, Titu Mir's *bidroha* of 1830–31, the Kol insurrection of 1831–32, the Santhal *hool* of 1855–56, the peasant revolts of 1857–58, the Pabna disturbances of 1873, the Kunbi riots of 1875, and Bisra Munda's uprising of 1899–1900. These movements reflect the elementary aspects of "negation," "ambiguity," "modality," "solidarity," "transmission," and "territoriality," all of which represent peasant attempts to struggle against their superordinates by rejecting the consciousness of inferiority, by ambivalently pursuing individualistic careers of crime or collective acts of resistance, by selectively confronting their superiors, by uniting against a common enemy, by forging bonds through signs and symbols—often of a religious nature—and by defending their sense of territoriality.

Guha's theoretical bases are the writings of Marx and Mao; especially important are the ideas of Gramsci regarding subalterns and consciousness. He also skillfully integrates into the account instructive comparisons with European peasant movements.

This study has to negotiate some untidy data, as does any highly synthetic and interpretive work tracing a grand pattern out of a complex past. Although three of the eight movements it highlights were tribal, it does not tackle adequately the question whether tribes are inherently more cohesive in organization than the castes and classes, which comprise the bulk of Indian society. In his accounts of the Pabna conflict between a largely Muslim peasantry and Hindu landholders or the Kunbi disturbances between peasants and "foreign" moneylenders (the local ones were not targets), Guha pays insufficient attention to religious and ethnic dimensions. Nor can he indiscriminately refer to the assortment of people who raised the banner of rebellion in 1857–58 as supporters of a peasant revolt. And for one who so brilliantly dissects the religious idioms of peasant dissidence, he seems uncharacteristically dismissive in relegating such manifestations to "false consciousness."

The "overdetermination" of the book results in part from the need to counter the prevailing distorted portrayals of the peasantry, in part from Guha's concentration on "seismic upheavals" and not on more routine experiences. But a good measure of it also stems from his insistence that the

British creation of "a highly centralized state . . . brought into focus the refracted moments of semi-feudalism in the countryside in a manner unprecedented in Indian history. And one of its direct consequences, that is, the fusion of the landlord's and the moneylender's authority with that of the sarkar [government], was what provided insurgency with the objective conditions of its development and transmission" (p. 226).

None can question, though, that this book is a seminal contribution to the discourse on peasants generally and South Asian subalterns specifically. Along with the recently recognized "moral" and "rational" peasants, we must now consider their "insurgent" comrades.

ANAND A. YANG
University of Utah

ASHIS NANDY. *The Intimate Enemy: Loss and Recovery of Self under Colonialism*. New York: Oxford University Press. 1983. Pp. xx, 121. \$16.95.

In *The Intimate Enemy* Ashis Nandy explores the psychological costs of colonialism to both ruler and ruled, with particular reference to India during the past two centuries. This is a large subject and one frequently dealt with in various ways by others, but by drawing on a wide range of disciplines this subtle and provocative book goes a step beyond what has been attempted before. In 1980 Nandy published *Alternative Sciences: Creativity and Authenticity in Two Indian Scientists*, which explored the difficulties of being both culturally Indian and a modern scientist. Now Nandy has written an essay addressing in much broader terms the questions posed poignantly by the lives of those two scientists, Srinivasan Ramanujan and Jagadis Chandra Bose. In his new book Nandy again makes effective use of two contrasting biographies: of Rudyard Kipling, whose memories of an idyllic Indian childhood had to be repressed in order for Kipling to pass muster as a real Englishman, and of Aurobindo Ackroyd Ghose, a Bengali whose Westernized parents gave him an English middle name and sent him to be raised in England free of contact with his ancestral language and culture and whose subsequent transformation into the mystic revolutionary Sri Aurobindo was a way of coming to terms with this severe upbringing. In Nandy's view, both Kipling and Sri Aurobindo are victims who reacted against but never quite transcended their colonial childhoods.

Nandy's historical explorations pose a question for the future: How much, if anything, will India retain from its long bout with British domination? British culture was forcibly inculcated into some and voluntarily embraced by others, either because it seemed better than what India already had or be-

cause it could be used to oppose Britain better. Now in the postcolonial dawn the modern West is paradoxically part of India's *history*. If modern ways survive, Nandy believes, this will only be because they are somehow blended with India's ancient culture. Nandy advocates "an ethically sensitive and culturally rooted alternative social knowledge . . . already partly available outside the modern social sciences" (p. xvii). Nandy contends that Indians must first come to terms successfully with their own distinctive past before they can borrow creatively from other countries.

Nandy's book exemplifies the synthesis he proposes, being a sophisticated blending of insights drawn from Western psychology as well as from India's literary classics and recent historical experience. *The Intimate Enemy* is compressed and allusive. But, for the reader who is not lost along the way, Nandy's work is exhilarating, foreshadowing an India that will be a dynamic blend of the best in East and West.

FRANCIS G. HUTCHINS
Wellesley, Massachusetts

WILLIAM C. KIRBY. *Germany and Republican China*. Stanford: Stanford University Press. 1984. Pp. viii, 361. \$35.00.

This book focuses on the interaction of China and Germany between 1928 and 1938. This interaction involved Chinese exports of critical raw materials, such as tungsten and antimony, and imports of arms, military and industrial expertise, as well as ideology and culture. Under German influence China also established state-controlled heavy industry away from treaty ports as a path to modernization, especially military modernization.

This multifaceted exchange was made possible by the confluence of favorable military, economic, ideological, and personal factors in both countries. William C. Kirby maintains that only after being stripped of imperialist pretensions and negotiating with China on an equal footing did Germany emerge as a preferred partner, forging ahead of Great Britain and the United States. This successful, if short-lived, partnership ended with Hitler's return to *Weltpolitik*. In the period of cordial relations, however, even national socialism as understood by Chiang Kai-shek and his entourage was welcomed by certain Kuomintang officials. In this light the New Life movement is seen as a failed attempt at mass mobilization.

On the German side, the major architects of these developments included Max Bauer, Hans von Seeckt, and Alexander von Falkenhausen. On the Chinese side, they were foreign-educated technocrats and intellectuals like Chu Chia-hua, Weng

Wen-hao, and Yu Ta-wei. Weng is described as a man of intelligence, integrity, and patriotism. He decided to serve in Chiang's government in 1932 when he was entrusted with the task of mapping out a new industrial strategy.

Many episodes of this decade of Sino-German relations are fascinatingly told, such as the formation of the China-Studien-Gesellschaft in Germany, the genesis and expansion of the National Resources Commission in China, and the indispensability of enjoying good personal relations with the Generalissimo. Germany's success in China was largely attributable to the congeniality between Chiang and Bauer and later between Chiang and Seeckt, who was instrumental in the signing of a series of Haplo barter agreements.

This study has the merit of attending almost exclusively to bilateral Sino-German interactions, but it runs the risk of overstating the case. An informed reader will not uncritically accept the statement that Chiang's government had closer relations with Germany than with any other power (p. 3). One suspects that Chiang's ruling apparatus was more complex than is described in the book. Criticisms of the cost of Chinese militarization, though not absent, are rather muted. Also rather unconvincing is Kirby's assertion that the "best and brightest" scholars, scientists, and technicians converted to Chiang Kai-shek's cause around 1932 (pp. 99, 258).

These caveats aside, the study is meticulously and thoroughly researched. The author relies chiefly on German sources. If by comparison Chinese materials are rather thin, the reason is not his lack of application. It is because of the paucity of available Chinese archival materials. Much of the story is being told here for the first time. This is therefore a remarkable contribution to our understanding of Sino-German relations during the Nanking decade.

MADELEINE CHI
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LLOYD E. EASTMAN. *Seeds of Destruction: Nationalist China in War and Revolution, 1937-1949*. Stanford: Stanford University Press. 1984. Pp. ix, 311. \$32.50.

During its years in power, the Nationalist party of China (the KMT) was perennially critical of its own performance. The criticism came both from insurgent movements within the party and from the constituted leadership. Chiang Kai-shek himself often led the assault or encouraged it and near the end expressed an "unabashed admiration" (p. 207) for the organization, discipline, methods, and morality of the Chinese Communist party. This theme of self-criticism marks several of the disparate chapters of Lloyd E. Eastman's book about the Nationalist regime's later years, before the retreat to Taiwan.

Yet, he adds, the critiques did not touch on the fundamental structural flaws in the Nationalist operation, especially its troubled relations with, or disconnection from, the mass of the people.

A decade ago, Eastman enormously stimulated scholarly reevaluation of the KMT government with his brilliant book, *The Abortive Revolution: China under Nationalist Rule, 1927–1937*. As new works of high quality reach print (for example, those by Hsi-sheng Ch'i, Parks M. Coble, Jr., and William C. Kirby), it seems fitting that Eastman is the first to take us to the 1949 divide. We arrive somewhat breathless, following his carefully planned hop, skip, and jump through the years of war with the Japanese and the communists. Warning that his treatment must be topical, he discusses an instance of provincial assertiveness against the center (Lung Yun), surveys the government's relationship with the peasantry, describes two movements for party renovation (the Youth Corps and the Ko-hsin movement), analyzes the decay of the army and its debacle, offers an engrossing study of an ambitious effort in 1948 to salvage the collapsing national currency, and presents Chiang Kai-shek's own remarkable pronouncements on the causes of the catastrophe engulfing his government. The chapters do not build toward an embracing explanatory thesis, but they abound with insights and make several excursions into debates on assorted issues.

Among the controversies Eastman wishes to address is the charge that the inadequacy of American aid crippled Nationalist forces after 1945. Eastman argues that neither the KMT military nor Chiang Kai-shek (who should have known) thought at the time that American aid was a factor. He does hold, however, that communist access to surrendered Japanese weapons in Manchuria, courtesy of the Russians, greatly hastened KMT defeat. And he chides scholars for ignoring the importance of communist agents, strategically located in the Nationalists' military establishment. It was the KMT, not the American Department of State, that had a problem with infiltration.

Eastman does not ask us, after all is said and done, to recast the broad outlines of the prevailing textbook versions of the regime's demise. His recurring focus on the KMT's reformist self-criticism leads one to expect the argument that the party and its regime retained a capacity for renovation, which might have saved the day. Eastman discounts this potential, however, and understands the reformist impulse as an aspect of factional maneuver. The collapse of 1948–49 stemmed, in his view, from infirmities present from the original formation of the Nanking government, which was further weakened during the Japanese war.

ERNEST P. YOUNG
University of Michigan

JAY RUBIN. *Injurious to Public Morals: Writers and the Meiji State*. Seattle: University of Washington Press. 1984. Pp. xvi, 331. \$35.00.

Jay Rubin's book describes the efforts of the Japanese state to control literary publishing between 1868 and 1945. Three-fifths of the volume addresses the period 1905–12, an introduction is devoted to the years 1868–1905, and a conclusion handles the years 1912–45. Rubin seeks to illustrate a single theme throughout, namely, that the Japanese state used coercive methods to manipulate literature before the war. The prose is polished, and having the events of literary control recounted in one place will be helpful for scholars.

Rubin describes some of the controversial works and acts of writers like Nagai Kafū, Natsume Sōseki, and Tanizaki Junichirō. He discusses the Press Law of 1887 and its revision in 1909 and examines the rash of book bannings of 1909, the Kōtoku Shūsui trial of 1910, and the short-lived Committee on Literature of 1911 to 1913. He looks ultimately at accelerating government control under the Cabinet Information Committee of 1936 and at the Yokohama Incident of 1942 in which forty-nine writers were jailed for leftist leanings.

Although many of the details in Rubin's work are interesting, his account somehow does not transcend its list of events. Rubin presents with relish the naughty magazine pieces of the Taishō demimonde that roused the ire of officials; these are the most engaging and informative portions of the work. Then he relates the officials' sporadic action against such pieces, touching on the major events of this cycle over a seventy-seven-year period. Apropos of the appearance of naturalism at the turn of the century, Rubin introduces the notion that the focus on highly personal matters in fiction of itself conveyed individualistic values, thereby threatening loyalty to family and nation.

The narrative of all this, however, is stronger than its analysis. Rather than unfolding a new analysis, this book reiterates the point of view that the Japanese government before the Pacific War was wrongfully manipulative of literature, in the name of collective values and at the expense of liberal ones. This idea is not so much systematically examined as repeatedly suggested in edged rhetoric. Rubin's viewpoint may represent the beginning of a cyclical revival of the perspective, common in the 1940s and 1950s, that sought to indict Japan rather than understand it and indeed avoided understanding the better to indict. Rubin complains again and again of the government's "prescriptive, doctrinaire view" and of its committing thought manipulation "in its narrowest, most vicious form." It is perhaps ironic that Rubin's treatment, too, may leave something to be desired in the deployment of open-mindedness

to combat simple and satisfying, yet confining, stereotypes. There is surely much to be learned about the perceptions of the writer's responsibility in Japan, both on the part of writers and on the part of officials, and about conflict between their respective attitudes. But I am not sure Rubin's polished narrative has shown us much about the dynamics of this that we have not often heard before.

THOMAS M. HUBER
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UNITED STATES

PHILIP F. GURA. *A Glimpse of Zion's Glory: Puritan Radicalism in New England, 1620-1660*. Middletown, Conn.: Wesleyan University Press; distributed by Harper and Row, Scranton, Pa. 1984. Pp. xv, 398. \$29.95.

If by 1984 an inventory of published strictures on Perry Miller's great synthetic work on the American Puritans might have assumed the proportions of Roget's *Thesaurus*, yet his unparalleled dominance of the field continues to be manifest in the ritualistically "revisionist" routine of so many current additions to the literature. This is certainly true of those who since Miller's death in 1963 have labored to demonstrate variety and dissensus in Puritan-era New England in contrast to Miller's report of an abiding intellectual unity. That current in post-Miller scholarship that has made "monolithic" a damning catchword reaches something of a climax in Philip F. Gura's study of Puritan radicalism. *A Glimpse of Zion's Glory* elaborates at length and with admirable ingenuity two provoking theses: first, that "heterogeneity, not unanimity" (p. 7), actually characterized the colonies' religious life and, second, that the ideological self-image that gradually did take form was shaped less by a preexisting and positive theological consensus than by the necessity to neutralize a medley of radical demands for more sweeping religious and social reform.

In working out these ideas, Gura displays the most thorough and integrated knowledge of New England dissent from 1620 to 1660 yet attained by any historian. And, if a too common failing of the existing historiography has been the treatment of the American phenomena in isolation, a procedure often rationalized with inflated and untested claims to "American exceptionalism," Gura has labored long to place his radicals within their proper international context. The result is the most complete and satisfying treatment to date of the shadowy English antecedents and continuing affiliations of figures like Samuel Gorton, whose General Baptist connections are drawn persuasively, or William Pyn-

chon, whose ties to European and English Socinianism are revealed in greater detail than ever before.

Most readers of this work will come away convinced that dissent was more widespread, incessant, and influential than they had suspected, yet they may also suspect that the author permits the dissenting tail to wag the dog of Congregationalist Puritanism. The data presented do not necessitate the conclusion that religious heterogeneity was the rule in early New England society. Part of the argument to this effect rests on simple exaggeration of the radicals' impact, as in Gura's insistence that the insubordination of servants, anticlerical sentiments, and the Massachusetts deputies' drive to trim the magistrates' power were *primarily* manifestations of spiritist influence. But the more basic difficulty is a concept of Puritanism that obscures its classical meanings. Although Gura nowhere states his working definition, the narrative tends to focus on the subjective, iconoclastic "mystical strain" as the crucial "Puritan" axis. Puritanism thus construed was inherently unstable; inevitably, it generated antinomian and spiritist offshoots that then can be cited as examples of Puritan heterogeneity. Fuller attention to the sources and characteristic profiles of the larger movement will yield a different result. Puritanism, which began in the 1560s as an agitation against nonapostolic vestments, embodied essentially a quest for moral purity and primitive ordinances that, in a context of firm allegiance to international Reformed Protestantism, was controlled by a peculiarly intense concentration on the Protestant scriptures. Through a complex development, it did in time give rise to a psychologically rich pietism and this, in turn, to antinomian and spiritist responses, but these were no straight extensions of Puritan content. They bore many residues from the past, but in conscious theory they were acts of liberation from definitively "Puritan" restraints. They looked well beyond Reformed horizons to the subjectively freer world of the Radical Reformation. In this sense, recognition of an extensive radicalism in New England, although a greatly useful complement to Miller's focus on the dominant spokesmen, does not dispose of his perception that a true Puritan unanimity stood at the heart of the society.

THEODORE DWIGHT BOZEMAN
University of Iowa

KENNETH SILVERMAN. *The Life and Times of Cotton Mather*. New York: Harper and Row. 1984. Pp. x, 479. \$29.95.

Cotton Mather presents enormous difficulties for any biographer. First, there is his reputation. For the most part, Americans have detested the Puritanism Mather represented, so they have had little

regard for his life and his career. They remember him mainly as the minister who supported the Salem witchcraft trials and as the man Benjamin Franklin parodied as "Silence Dogood," a "biddyish seriocomic martyr eternally vaunting his tiresome benevolence and meekness" (p. 360). Second, there is the man himself. At a time when most colonials struggled to survive and lived lives mixing equal parts of tedium and terror, Mather gave a virtuoso performance as a Renaissance man. His career touched every facet of colonial life, and his mind tried to explore and explain much of human knowledge. Thus, his career and his life seemed to lack a center. The sum of the parts did not add up to any whole. Mather left his own story of his life, but he often hindered understanding by strutting, posturing, magnifying his strengths, minimizing his weaknesses, excoriating his enemies, and listing the numerous (and very real) travails that supposedly hindered him.

Because of these difficulties historians have kept their distance from Mather. When they have discussed him, they have usually endorsed Perry Miller's description of a besieged Mather trying somewhat pathetically to protect the "New England Way" in a hostile world. Kenneth Silverman gives us the first modern biography of Mather, and it is a very good one. His Mather is robust, not pathetic. His Puritanism, though besieged, is alive and well and coping nicely.

Religion and ambition were the keys to Mather's life and career. His religious training and his beliefs did not force him to turn his back on the world or reject it but to embrace it. His intense piety, his Calvinist theology, and his Congregational ecclesiology fueled his insatiable desire to understand both the visible and the invisible worlds. He shared with his Protestant and Puritan ancestors a common desire to reshape and reform men, women, and their institutions, and his optimism and his zeal were no less than theirs. His position as minister to Boston's and New England's most important congregation (as well as his membership in New England's most important ministerial family) led him inevitably into public affairs. It was expected of him. But he did more than was expected. He was not just a Puritan oligarch; he was a leader and an innovator.

Ambition fired his soul, even though he recognized how much ambition violated the humility he was supposed to exhibit. As an intellectual—and Mather was an intellectual—ambition bred greatness. He wanted a transatlantic reputation, and he achieved it. In public affairs, though, ambition often led Mather into disaster. A good and gentle man who was also vain and arrogant, he could be thoughtless and tactless and was capable of incredibly bad judgment. Thus, too frequently he became

involved in affairs that tarnished his ministry and opened him to public ridicule.

Mather's career did take an amazing number of turns and twists, and Silverman's narrative follows them carefully. The results are both enlightening and frustrating. For the first time we see the breadth of Mather's thought, and we see also how his interests changed as he matured. The young Mather was fascinated by the spirit world and by psychic powers and prophecy. The mature Mather, buffeted by public failures and personal tragedies, and acutely aware of the sufferings that dog persons great and small, devoted his life to doing charitable works. At the same time the shifts in Mather's thought and behavior are not always well explained, and important themes in his life are sometimes dropped without explanation. For example, in the early chapters Silverman makes much of the often-strained relationship between Cotton and his father, Increase, and of how the influence of the father shaped the character of the son. Incredibly, that story ends shortly after Cotton's ordination in 1685 even though Cotton and Increase shared the same pulpit until 1723.

Only a few weaknesses mar this fine book. Specialists will note that this is not an intellectual biography of Mather and that Silverman does not attempt to rewrite the intellectual history of the period. He does tell us some new things about Mather's mind, but he fails to reveal the significance of those discoveries. He gives us a superb portrait of the character of the man, however. He concludes that Mather was "the first unmistakably American figure in the nation's history" because of his "epic reach" and his "quirkily ingenious individualism" and because "he was the first person to write at length about the New World having never seen the Old" (p. 426). I think Silverman overstates the case for the "American" Mather. Instead, I think he has crafted a remarkable portrait of an Anglo-American genius. Mather's religion and his ambition kept his gaze fixed on England and on Europe. He spent much of his intellectual life interpreting the Old World for the New and the New World for the Old. His father, Increase, wanted to return to England to live. Cotton just wanted to be famous there. Both men shared a provincial mentality that was typical of that age of empires.

PAUL R. LUCAS
Indiana University

JON BUTLER. *The Huguenots in America: A Refugee People in New World Society*. (Harvard Historical Monographs, number 72.) Cambridge: Harvard University Press. 1983. Pp. viii, 264. \$25.00.

In 1685 Louis XIV revoked the Edict of Nantes, destroying any lingering hope that Protestantism might survive as a vital religious movement within France. Five years later, the million Protestants in France had dwindled to seventy-five thousand: most had converted, but about one hundred sixty thousand had become refugees throughout Europe, and, of these, a fraction eventually emigrated to the New World. The story of these exiles and their failure to maintain religious and social distinctiveness in diaspora is related in this important, well-wrought book on the Huguenot experience.

Jon Butler's thesis is that the reasons for Huguenot loss of identity lay in the nature of the Huguenots themselves—in their lack of ability to create a unifying religious structure—rather than in the seductiveness of any particular culture into which they moved. Butler uses four primary measures of assimilation: disintegration of the French Reform Church, entry into the economic system of the host community, participation in the established political process, and social assimilation through exogamy (marriage outside the ethnic community). In every category, in every community, Huguenot refugee cohesion was fatally weakened by 1710 and had virtually disappeared by the 1770s.

After a brief, but crucial, exploration of the European context providing a reference point for the principal focus on colonial America, the Huguenots of Massachusetts, South Carolina, and New York are analyzed in detail. Butler not only produces the first scholarly modern assessment of the size of the Huguenot population, but he also considers rural settlements as well as the principal urban centers of Boston, Charlestown, and New York City, contradicting traditional stereotypes of Huguenots as disproportionately wealthy urban merchants and artisans. He presents evidence that they spread across the class and occupational structure of their adopted colonies and helped to create the distinctive regional style of each. Thus, the Huguenots were Puritan Yankees in Boston, slave-owning planters in South Carolina, and cultural opportunists in New York. By the end of the book, it seems clear that for the Huguenots, at least, the eighteenth-century ethnic experience was one of assimilation.

The larger theoretical and methodological frameworks suggested here should provide useful signposts for historians who follow other ethnic trails, because several crucial questions are addressed and methods for answering them are presented. What difference is made by involuntary flight as opposed to voluntary choice? How are the conclusions changed by focusing on actual individual experience rather than apparent institution building? Is there a critical mass for maintaining identity? Does prejudice encourage or retard assimilation?

The principal shortcoming of *The Huguenots in America* is Butler's own insistence on the special, idiosyncratic nature of the Huguenots. Space devoted to a detailed narrative of Huguenot ministers often seems to be at the expense of an exploration of more universal indicators of assimilation, such as language retention, naming patterns, and residential distribution. In the end, the author somewhat underestimates the important contribution he has made to an understanding of eighteenth-century immigration and ethnicity. Interested scholars should not make the same mistake.

STEPHANIE GRAUMAN WOLF
University of Delaware

BETTY WOOD. *Slavery in Colonial Georgia, 1730–1775*. Athens: University of Georgia Press. 1984. Pp. x, 254. \$22.50.

In this useful monograph Betty Wood considers the founding of Georgia as a reform colony to rehabilitate poor and criminal elements from Britain and to create a virtuous, middling society free from the defects of luxury and lethargy concomitant of slavery. The trustees designed a colony in which great inequalities of wealth were to be avoided. In addition, Georgia was settled as a security measure on land disputed with Spain. The planners therefore desired a denser settlement pattern than in other colonies and placed limits on land holdings, provisions that fitted the social as well as military aspects of the settlement. The decision to ban slavery in Georgia, then, had to do with an interest in the welfare of whites and the British empire rather than with a concern about the plight of blacks. Nor was there any desire that the colony serve as an outpost of emancipation affecting other colonies. The trustees' aims regarding slavery were strictly local and limited.

Unfortunately, the trustees subscribed to what Wood calls the "Georgia Myth"—the idea that its land was unusually fertile and that cultivation of cotton, vineyards, and other crops or the production of silk and other commodities would be easy or effortless. When these hopes proved unfounded, heightened expectations magnified subsequent difficulty. Proponents of the trustees' plan blamed economic hardship on the aversion of settlers to sustained labor and suggested that, if sufficiently motivated or otherwise qualified colonists would immigrate, the project was bound to succeed. (Indeed, Wood indicates that the first settlers neither had adequate skills nor were of the proper age group to best inaugurate a new colony.) Moreover, the trustees' supporters argued, slavery would be unusually dangerous in a borderlands settlement designed to maintain a defensive posture. Oppo-

nents of the trustees argued that whites were unsuited to labor in Georgia's climate, that white indentured servants were difficult and expensive to maintain, and that only black slaves could guarantee the success of the Georgia venture. The example of South Carolina and the interest of prominent South Carolinians in Georgia as an area in which to expand their successful system provided support for the position of malcontents.

The trustees were further hampered in their design by the need to maintain parliamentary funding. The necessity to illustrate that the venture was in the national interest meant that they could colonize only those Englishmen who were not useful in the mother country (which often meant they were not the best choice for building a new settlement either) and also demonstrate that the colony under the "Georgia Plan" had a reasonable chance for success. When the malcontents got serious parliamentary attention and the trustees were unable to extenuate an inadequate progress, the idealistic social aims of the colony were on the road to extinction.

Roughly the first half of Wood's story concerns the failure of the trustees' plan and the subsequent vain attempt to provide a more humane system of involuntary servitude once the decision was taken to capitulate to apparent economic necessity. The second part deals with the institution of slavery itself. As the attempt to keep slavery out of Georgia collapsed partly under the influence of South Carolina, so did the attempt to establish a variant system. Thereafter, slavery in Georgia closely resembled the institution of its northern neighbor.

Based on traditional archival sources, apparently mostly published, in Georgia and, perhaps to a lesser extent, in England (though it is hard to tell since there is no bibliography), the book is better on the controversy over slavery than on slavery itself. The documents better suit it to deal with the former. For those concerned with the latter, however, the second part of the book is more interesting. What Wood does she does well, and the book's limitations are largely the limitation of the documents. Official papers are often an inadequate reflection of reality. For example, when Wood relates that a Georgia law of 1755 required that planters take their guns to church, one wonders whether they, as was common in South Carolina, left them outside with their servants. Considering the slave population solely from official pronouncements leaves even more to be desired. Despite these reservations, however, Wood makes a solid contribution to the study of colonial America.

DANIEL C. LITTLEFIELD
Louisiana State University

WILLIAM E. FOLEY and C. DAVID RICE. *The First Chouteaus: River Barons of Early St. Louis*. Urbana: University of Illinois Press. 1983. Pp. xi, 241. \$22.50.

The First Chouteaus is the biography of Auguste and Pierre Chouteau who lived in St. Louis in the Spanish and American eras. Also prominent in the study are their mother, Madame Chouteau; Pierre Laclède Ligest, with whom she formed a liaison and who was the father of Pierre Chouteau and the younger Chouteau children; and several sons of Auguste and Pierre who continued the commercial activities of their fathers. Shortly before word arrived of France's transfer of Louisiana to Spain, Laclède, a French merchant, took his acquired family from New Orleans to Upper Louisiana, where he and Auguste founded St. Louis in 1764 and helped to make it the emporium for the Missouri River fur trade. After Laclède's death in 1778, Auguste kept the family in the forefront of the St. Louis merchant community. In addition, Auguste and his brother Pierre were land speculators, militia officers, advisers to the Spanish and American governments, community leaders, and public officials. They possessed power and wealth in St. Louis until they died well into the nineteenth century.

The authors, William E. Foley and C. David Rice, focus in the Spanish era on Auguste, who predominated as St. Louis's most important merchant, government consultant, and friend of all Upper Louisiana's lieutenant governors. Through the 1770s and 1780s, both brothers participated in the trade monopolies the Spaniards generally employed to keep prices of trade goods high and Indian tribes dependent on their merchandise. When foreign encroachment resulted in the decline of the Spanish-Indian trade, Auguste convinced the Spanish governor, baron de Carondelet, to grant him exclusive commerce with the powerful and numerous Osage nation in 1794 in return for keeping that troublesome tribe under control. Chouteau retained the lucrative monopoly for eight years but continued to trade with the Osages for many years after the Americans took possession of Louisiana.

With this change in ownership in 1804, Pierre Chouteau rises to the forefront in the biography, although Auguste continued to exercise a significant role in commercial, civic, and governmental affairs. Pierre's firsthand knowledge of the Upper Louisiana tribes earned him appointment as Indian agent for the United States government, but the vestiges of French and Spanish methods in his approach to Indian affairs occasionally made him suspect to American authorities. The authors draw in part from their previously published articles in this section of their study.

The authors synthesize their story even more for the post-1812 period, but they nevertheless ade-

quately explain the overall participation of the Chouteaus in commerce, real estate, and civic affairs. The family showed an ability to survive changes in governments and officials and retain its prominence among St. Louis's leading citizens.

The vast amount of information presented by Foley and Rice, drawn from numerous American archives and libraries, enhances their study. An examination, however, of Spain's Archivo General de Indias would have added significantly to understanding the Chouteaus in the Spanish era. But this point aside, the authors' sympathetic biography of Auguste and Pierre Chouteau surpasses earlier works, and it is likely to stand for some time to come.

GILBERT C. DIN
Fort Lewis College

E. WAYNE CARP. *To Starve the Army at Pleasure: Continental Army Administration and American Political Culture, 1775-1783*. Chapel Hill: University of North Carolina Press. 1984. Pp. xiii, 306. \$29.00.

This important book on army administration during the American Revolutionary War joins recent works by Erna Risch, Mary C. Gillett, Paul K. Walker, and others in adding to or superseding the earlier scholarship of Louis C. Hatch. But E. Wayne Carp's volume stands by itself in sophistication of argument, thoroughness of research, and quality of writing. The author has made an original contribution to the growing field of Revolutionary military history as seen from the perspective of the whole society rather than from army headquarters or from the general's saddle. Consequently, his study bears favorable comparison with others in recent years by scholars such as John Shy, Don Higginbotham, Charles Royster, and Lawrence Delbert Cress, all of whom in sagacious and insightful ways have illuminated the intimate connection between sociopolitical realities and the nature of military institutions during the years 1775-83.

Carp's primary argument is that Continental Army administration suffered from inefficiency that endangered the Revolution's very success, because Americans were thoroughly imbued with a political culture, formulated during years of colonial practice, that stressed localism, deferential politics, and real Whig ideology. Paradoxically, as Carp notes, these values, which on the one hand "propelled the colonists into war against the strongest imperial power in the world," at the same time "prevented them from waging war effectively." Therefore, "time and time again . . . individual citizens, local magistrates, state officials, and members of the Continental Congress refused to accept that a well-equipped army was necessary to win the military victory that would guarantee American indepen-

dence; that the prosecution of a war required spending money for supplies and manpower; and that long-held and cherished principles of divided political power, local autonomy, civil control over the military, and fear of power would have to give way if Americans were to win the war and preserve liberty" (p. 14).

This quotation admirably demonstrates the succinctness and lucidity of the author's arguments. Equally valuable is the remainder of his book, which convincingly demonstrates by example and proof the truth of these summary propositions. The volume itself is attractively designed and includes useful illustrations, an appendix, bibliography, and index. In sum, Carp's book takes its place in American Revolutionary scholarship alongside the handful of seminal works in that field during the past few years and establishes its author as an important historical thinker.

PAUL DAVID NELSON
Berea College

GARRY WILLS. *Cincinnatus: George Washington and the Enlightenment*. Garden City, N.Y.: Doubleday. 1984. Pp. xxvi, 272. \$18.95.

Garry Wills is unfailingly interesting. *Cincinnatus* is an exploration of the way "educated artists and propagandists shaped a deliberately didactic image of the nation's first great leader" (p. xxi). The new nation needed symbols of stability, and Washington willingly met the need. He embodied the Enlightenment's conception of political heroism. Wills organizes the book around three central acts in Washington's public life—his resignation as commander in chief of the Revolutionary army, his support of the new constitution in 1787, and his relinquishment of the presidency in the Farewell Address—and explores the symbols through which Americans understood the meaning of each of the three.

Washington gained power "by his readiness to give it up" (p. 23). Marcus Cunliffe long ago argued the necessary and central importance of Washington's charismatic role in the founding of the new nation. Unlike other charismatic leaders such as Cromwell, Stalin, and Mao, Washington's charisma derived from his willingness to subordinate himself to the Continental Congress, which had given him his extraordinary powers during the Revolution and to which he surrendered those powers when peace was finally made. The image was Roman: Cincinnatus, called in emergency by the state from his plow, eager to return to his farm when order was restored. The role of reluctant subordinate created obvious problems for Washington at the Constitutional Convention, since it was

illegal and against the procedures required by the Congress for which Washington had been only an agent. Then, as president, Washington again relinquished power in what we now call the Farewell Address, written months before its appearance, and acted in a tutelary role to American citizens.

What is interesting, though, in Garry Wills's exploration of the meaning of these moments in Washington's public life—and, far more important, of their meaning for a legitimate, virtuous, and republican order at the start of the United States of America—is the explication of the images through which these moments were presented.

Since this review is written for professionals and not the general reader, one virtue in *Cincinnatus* is worth stressing. The busy and industrious specialization of scholarly research is justified, ideally, by the notion that the bricks will somehow make buildings. Wills, whose reading of the meaning of American political history is at times quirky, has read the secondary literature in the field of art history concerning the conventions that establish the genres in which painters work. What he has brought back from that reading enables him to amplify our understanding of the meaning of political history, his own specialty.

The difficulty in reviewing this book is that Wills deals with images. *Cincinnatus* is copiously illustrated, and the seventy-nine black-and-white illustrations and six color plates are the heart of the book's pleasure and instruction. His prose moves around and about them, playing one image off against another. In the juxtaposition of words and image, one comes to understand how the work of artists, sculptors, and lithographers are implicated in the political culture in which they work. That is no small achievement. But the greater achievement is a richer understanding of why George Washington is a monument in our understanding of the meaning of the American republic.

JOHN WILLIAM WARD
American Council of Learned Societies
New York, New York

RALPH KETCHAM. *Presidents above Party: The First American Presidency, 1789–1829*. (Institute Bicentennial Studies on the Constitution and Early American Law and Government.) Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Virginia. Pp. xiv, 269. \$24.95.

Titles and subtitles sometimes tell us all too little about the actual themes of the books we read, but such is decidedly not the case with Ralph Ketcham's new study. This book focuses primarily on what the first six presidents thought about the nature of the

office they held and why they thought as they did. Despite all the differences of philosophy, temperament, and political style, the six were, according to Ketcham, essentially unified in their commitment to a nonpartisan presidency. Hence the title. Indeed, Ketcham contends that these men were so like each other in their most fundamental view of the office and so unlike their more blatantly partisan successors that the first six administrations can be seen as a single stage in the development of the presidency. Hence the subtitle.

What unfolds is an extraordinarily well-informed intellectual history of one important aspect of early American politics. Ketcham does an excellent job of tracing the classical, Christian, and modern origins of the idea that the basic responsibility of the leader is to see to the welfare of the whole people. The author stresses in particular Henry St. John, Lord Bolingbroke's famous *Idea of a Patriot King* and the influence of the views contained therein on early American republicans. Ketcham's is as good a brief study of this matter as there is around. In the second major part of the book, he deals insightfully with each of the early presidents, graphically showing "the ambivalence and agony felt by each . . . as they faced the fact of burgeoning party activity on all sides while seeking earnestly to fulfill an ideal of leadership having no valid place for it" (pp. 163–64) and showing too "the triumph under Jackson of a conception of leadership tied to a positive idea of party" (p. 130). The third, final, and least unified segment of the work treats what Ketcham calls "Republican Dilemmas." Here, in an attempt to shed additional light on such themes as leadership, virtue, republicanism, partisanship, commerce, and individual and national ambition, the author analyzes the thought of Thomas Jefferson, Benjamin Franklin, and Alexander Hamilton, the last of whom Ketcham regards as "the American of his time perhaps most richly endowed with natural qualities of leadership" (p. 164). Though conceptually rich, the third segment does not fit neatly into the rest of the book. Indeed, it seems almost to be another study. Important points in the third segment could have been—and should have been—incorporated into the previous two.

In addition to these organizational difficulties, the ultimate questions remain. Did the first six chief magistrates really honor the presidential ideal they espoused as much in the keeping as in the breach? And do the six really constitute a meaningful group at all, one cut off somehow from those presidents who came after them? Although Ketcham has articulated his positions in a strong manner, numerous primary and secondary sources can be called in to bolster other perspectives. But, even if Ketcham is not entirely persuasive, his study will nonetheless

have the great virtue of causing all serious readers to rethink their own views on these things.

ROBERT P. HAY
Marquette University

PAUL R. PASKOFF. *Industrial Evolution: Organization, Structure, and Growth of the Pennsylvania Iron Industry, 1750–1860*. (Studies in Industry and Society, number 3.) Baltimore: Johns Hopkins University Press. 1983. Pp. xx, 182. \$22.00.

This slender volume by Paul R. Paskoff traces the history of the Pennsylvania iron industry during the century before the Civil War, chronicling the transformation that the leading firms of the industry underwent as they entered the modern era. Most of the industry, however, continued along traditional lines. As Paskoff says at the close of his narrative, "Most of the men who owned iron firms in the 1850s were not unlike the businessmen of the 1750s" (p. 132), and the iron business was not very different either. This era, clearly, is the last for which such a statement could be made.

The book grew out of work done at the Eleutherian Mills-Hagley Foundation, and it reflects the interest of that institution in the local history of the colonial and early national period. The book is noteworthy in its intensive use of a variety of sources. Paskoff draws on the manuscript sources collected at the foundation and elsewhere, but he does not stop there. He relies heavily as well on published compilations of data about the industry, producing a quantitative as well as a qualitative picture. He is to be congratulated on his ability to find new information in these sources that are familiar to quantitative economic historians.

The picture that emerges is not unexpected. The industry responded more to changes in its economic environment than to technological changes, which consisted only of minor improvements and refinements. The dominant impression is of stasis until the end of the period surveyed when rapid technological progress can be seen.

Several interesting aspects of this picture are revealed by Paskoff as he paints in the details. He gives more attention to the varieties of business forms used than previous authors, and he discovers that ironworks run by companies were not in general more progressive than those operated by individuals or partnerships (p. 101). Using quantitative evidence, he also shows vividly how perilous the condition of the industry was in the 1840s. The iron industry, hard hit by the depression of industry and construction after the Panic of 1837, turned first to political action—as testified by the several memorials to Congress that provide the source material for Paskoff's analysis—and then to technical improve-

ment. It is clear that the process of industrialization was not simply smooth sailing for those involved.

Two intriguing findings raise questions for future work. First, Paskoff notes a *negative* correlation between man-days worked and output in one forge of the late eighteenth century (p. 26). His discussion of this only makes it more tantalizing. Second, there is a *positive* correlation between furnace size and the interest rate in the late antebellum period (p. 127). This suggests strongly that the role of the interest rate was not direct but rather acted as a proxy for general economic conditions. Again, Paskoff's discussion opens up the issue, suggesting avenues for future research.

This is a book to be recommended to anyone seeking to know industrial conditions on the eve of industrialization.

PETER TEMIN
Massachusetts Institute of Technology

G. EMLIN HALL. *Four Leagues of Pecos: A Legal History of the Pecos Grant, 1800–1933*. (New Mexico Land Grant Series.) Albuquerque: University of New Mexico Press. 1984. Pp. xxi, 367. Cloth \$24.95, paper \$14.95.

During the Spanish colonial period in New Mexico the Pecos Pueblo Indians were recognized as the legal owners of eighteen thousand acres of land defined as the Pecos League. In 1837, ten years before the conquest of New Mexico by Stephen Watts Kearny and his Army of the West, the ten surviving members of the Pecos Pueblo believed that their property rights were being whittled away by the Mexicans. Despite validation of their traditional land claims by the Mexican government, the Pecos Pueblo relocated to Jemez Pueblo, where their descendants remain today. The stage was thus set for one of the more complex land grant cases ever to plague the court system of the United States. The Pueblos had abandoned the land, but not their claims. Later, when *Hispanos* and whites sought to exploit the rich water resources and achieve legal possession of the land, the Pueblos reasserted their claim to ownership.

Land grant and water rights issues have continued to persist as some of the most important causes of friction between Indians, *Hispanos*, and whites from the eighteenth century to the present day. Consequently, there has always existed a historical need for systematic examination of these major problems for an understanding of the social, economic, and political relations of the three groups.

R. H. Tawney once suggested there was a want for historians to put on their seven league boots and explore land changes in each shire of England in order to understand the rise of a new gentry class.

William A. Keleher later insisted there existed a similar deficiency in comprehending the land grant problems in New Mexico.

G. Emlen Hall's work, a volume in the New Mexico Land Grant Series edited by John R. Van Ness, is of as high a quality as the prior general study by Victor Westfall, *Mercedes Reales: Hispanic Land Grants of the Upper Rio Grande Region*, and the restricted examination by Keleher, *The Maxwell Land Grant*. Like Keleher and two earlier historians of New Mexico, L. Bradford Prince and Ralph E. Twitchell, Hall is a lawyer-historian. His book provides a clear understanding of the conflicts between rival land claimants and of the numerous reversals of opinion by lawyers, judges, and administrators to explain the legal history of the Pecos grant. His sources include primary and untapped documents spread from New Mexico to New York. In addition, Hall lived ten years in Pecos and used the extremely rich oral history sources of information there, including interviews with the Zamora, Ortiz, Varela, and Roybal families who yet reside at Pecos. His notes include all the major works and articles of the past, including the important ones by Myra Ellen Jenkins.

There are a few minor faults to be found with the book—*east* should read *west* (line seven from bottom, p. 69), and *Monroe* should be spelled *Munroe* (line 12 from bottom, p. 84). Also, a glossary of terms would be of immense value to the Anglo reader.

Hall has made a major contribution to the history of New Mexico, and his book is a model of local history as well as a study of land encroachment and the legal relationships between people and land in New Mexico. It should be read by all serious students of ethnic history in the Southwest.

ALVIN R. SUNSERI
University of Northern Iowa

JOHN R. FINGER. *The Eastern Band of Cherokees, 1819–1900*. Knoxville: University of Tennessee Press. 1984. Pp. xiv, 253.

John R. Finger's book is the first of a projected two-volume study of the North Carolina Cherokees. Since it is the first book-length study of the tribe, this work is a significant contribution to the literature on that long-ignored group and therefore deserves the attention of both scholars and casual readers.

Finger traces the origins of the Eastern Band of Cherokees to 1819, when, under treaty stipulations, a group of Cherokees separated from the tribe proper and settled on private reserves in North Carolina. These and the others who later joined them managed to escape forced removal by their insistence on treaty rights, their assistance in round-

ing up and executing resisters such as the legendary Tsali, and toleration on the part of North Carolinians. During the fifty years following removal of their kinsmen, their existence was precarious as they struggled constantly to acquire and retain a land base, to define their status as a tribal entity and as citizens of North Carolina, and to clarify their relationship with the state and federal governments. Throughout this period, the Eastern Cherokees also suffered the additional burdens of grinding poverty, pressures to assimilate or to remove to the West, ill-advised involvement in the Civil War, manipulation and fraud on the part of government agents and others, and a bitter political factionalism that pitted the more generally traditional inhabitants of Qualla Boundary against the mixed bloods on other reserves. The narrative carries their history forward to 1900, three years after the federal courts finally recognized the Eastern Band as a tribe and nullified their rights as citizens of North Carolina.

Despite Finger's prefatory insistence that he had tried to "return, ultimately, to the Cherokees" (p. xiii) in his narrative, the work remains a policy history. It is whites or near whites, not Cherokees, who dominate the narrative. For instance, over half of the work focuses on the activities of William Holland Thomas, a white man who assisted the Cherokees in successfully resisting removal, who was responsible for the acquisition of most of their land base, and who unfortunately led them into the Civil War and the tribal factionalism that followed. After Thomas ceased to be an active participant in Cherokee affairs, his legacy persisted, and it pervades the narrative to the end. The book could well have been titled "William Holland Thomas and the Eastern Band of Cherokees." On the other hand, Cherokees like Euchella, Enola, and Yonagaska remain shadowy figures at best. Those Cherokees who emerge in clearer relief in the narrative—mostly mixed bloods such as John Ross, James Taylor, George Bushyhead, Nimrod J. Smith, and James Blythe—do so in terms of whatever posture they assumed in relation to federal or state policy and actions. We are told that, in maintaining a tribal identity, the Cherokees also managed to salvage some of their traditionalism, but the narrative offers only an occasional insight into what form that traditionalism took.

The problem, which Finger aptly identifies (p. xii), in telling the Indians' story is the bias of the sources. Since most Cherokees were not literate, they left no records. Thus, the historian must rely on what is available: in this case, federal and state records and private papers, particularly those of Thomas. But Finger works those sources well and out of them has produced a narrative that is read-

able and that puts the Eastern Band of Cherokees as a tribal entity into a clear, historical perspective.

DANIEL F. LITTLEFIELD, JR.
University of Arkansas,
Little Rock

RICHARD WHITE. *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos*. Lincoln: University of Nebraska Press. 1983. Pp. xix, 433. \$26.50.

Richard White's study of the collapse into "dependency" of three native American subsistence economies represents the best kind of interdisciplinary effort. Here ideas and approaches from several fields—mainly anthropology, history, and ecology—are fruitfully combined in one inquiring mind closely focused on a related set of large, salient problems. White sets out on several paths. Creatively improving on earlier versions of thinking about economic and political dependency is only one such; balancing a now standard overemphasis on material and economic factors and penetrating processes of change with cultural and social insights is another. Explaining how societies based on kin ties and reciprocal or redistributive exchange modify their environments while exploiting them productively is one more. Sharply describing and accounting for sequences of induced political-economic changes adds to the list of issues he addresses in this powerful study that is certain to be influential.

The main aim of this book, however, is to identify the key factors that make intelligible the economic breakdown and environmental degradation of three native American societies. White uses a format employed by anthropologists called "close, controlled comparison." But he concentrates neither on the changing adaptations of groups that are closely related culturally and linguistically nor on those that cope with induced changes in similar, neighboring environments. Indeed, exactly why he selected the Choctaw, Pawnee, and Navajo for his case studies the author never explains. Notwithstanding, although two thousand miles and vast cultural, environmental, and historical differences separate the one from the others, significant likenesses are there, making the comparison valid and extraordinarily productive of insights. The control in White's study is exercised by his highly disciplined, generally consistent analysis, by the application of a productive body of powerful constructs and hypotheses, and by his impressive command of a wide array of data.

What the Choctaw, Pawnee, and Navajo shared was a set of similar subsistence adaptations and social structures. All exploited diverse microenvironments with some mix of horticulture, hunting, and foraging. That all three were also matrilineal

societies is a salient social structural fact that is given too little attention by this author.

Some striking insights deserve special mention. The critical factor producing collapse of the old cultural-economic-political order, obviously, was incorporation of these societies into a European-dominated and -dictated market system, not the application of military force. The Choctaw and Pawnee never seriously contested Americans and were never defeated by them; even the Navajos' brief military brush with Americans cannot explain their much later collapse. One tactic regularly employed by Euro-American merchants, when faced with modest and nonrecurring demand for manufactured hard-goods, was to create demand for consumables, for example, to press alcohol on these native societies. Yet their responses to this were remarkably diverse. The Choctaw soon succumbed, quickly destroying available deer herds in exchange for rum. But the Navajo long avoided such addiction, jointly convinced of the wastefulness of swapping hard work for instant pleasure, and authoritative Pawnee chiefs successfully implemented a policy of trader management and prohibition, and thus, through leadership, they blocked this threat to community well-being.

The merits of *The Roots of Dependency* are many, its weaknesses few. Unfortunately, like many who should know better, the author favors the conventional racial label "white" as designator for the diverse peoples who brought new perspectives, products, and economic practices to North America. Also, like many, he favors the undefined, misleading catch phrases "nation" and "colonialism." Similarly, like others, he is unable to view recent immigrants into North America with the alertly detached, humane sympathy he so generously and effectively bestows on Choctaw or Navajo. To characterize Anglo-Saxon and Scots-Irish workingmen cum traders who pursued Choctaw customers while seeking their own forms of security as "knaves, murderers, rapists, and all-purpose reprobates" (p. 56) displays elitist class prejudice, not scholarly understanding, and this predilection leaves an important dimension of the story of European-Indian contacts again unanalyzed. The account of the effects of the Bureau of Indian Affairs's Navajo erosion control and sheep reduction program is almost entirely partisan and overly emotional, displaying little understanding of the complexities of introducing innovative regional planning programs in the rapidly developing Southwest (and other parts of North America). Finally, in an otherwise temperate and balanced exposition, the last chapter unfortunately degenerates by putting the blame for this program on one participant, John Collier. The assault on Collier violates the promise made in the introduction: the Navajo do emerge as environmental saints,

contrasted with "white" environmental demons. Otherwise, this is a very sophisticated study, a "best read" in Indian history.

JAMES A. CLIFTON
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Green Bay

FREDERICK E. HOXIE. *A Final Promise: The Campaign to Assimilate the Indians, 1880-1920*. Lincoln: University of Nebraska Press. 1984. Pp. xvi, 350. \$25.95.

Frederick E. Hoxie's book is the first devoted specifically to the campaign to assimilate the Indians in the forty years between 1880 and 1920, with the major attention given to the twentieth century. The author limits his discussion to "white leaders," who were mostly in the executive and legislative branches of the federal government or in anthropology; on occasion he mentions white reformers. Because he believes that most of these leaders defined national Indian policy in terms of the trans-Mississippi West, he omits the East and Oklahoma, considering the latter to be a special case with little national applicability. The book contains interesting chapters on changes in anthropology, land policy, education, and citizenship. These emphases are reflected in the sources, which consist of government archives and documents, papers of persons active either officially or unofficially in Indian affairs, legal cases, and newspapers, periodicals, and books from the period.

Assimilation is a slippery subject made more slippery by Hoxie's loose use of the term. The book's thesis is that the type of "total assimilation" advocated by white leaders in the 1880s (one definition given is "the incorporation of independent Indian landowners into American society on an equal footing with their fellow citizens" [p. 152]) had by 1920 been transformed into a radically different type of assimilation in which Indians as a group "would remain on the periphery of American society, ruled by outsiders who promised to guide them toward 'civilization'" but did not expect them "to participate in American life as the equal of their conquerors" (p. 341). At another point Hoxie refers to assimilation much more generally as "greater contact between the races" (p. 173). The book often describes accurately shifts in thought as well as behavior toward Indians, but to call all these attitude "assimilation" is confused and confusing.

Two major elements in the campaign for assimilation are largely neglected. The first consists of the missionaries and white reformers in the Indian rights organizations who helped to define the terms of Indian assimilation and to shape public attitudes toward native Americans while serving as their chief friends and intermediaries in court. The second is the Indians themselves. The author explains his

omission of the Indians' role by pointing out that the book's "primary focus is federal policy as a reflection of changes in white society" (p. xiv). This unfortunately transforms a relationship that should be examined into an assumption that is not.

Despite such problems historians will find much that is useful and valuable in *A Final Promise* and will welcome the book into the growing body of historical writing on twentieth-century Indian-white relations.

HAZEL WHITMAN HERTZBERG
Columbia University

MICHAEL BIRKNER. *Samuel L. Southard: Jeffersonian Whig*. Cranbury, N.J.: Associated University Presses, for Fairleigh Dickinson University Press. 1984. Pp. 269. \$32.50.

As the Founding Fathers gathered in Philadelphia in the summer of 1787, a boy was born into republican simplicity in rural New Jersey. In a public career that spanned the next thirty years, Samuel Southard came to represent the values of a new generation of Americans—the urban, secular, and money-oriented men of the future. Rejecting the Jeffersonian-Jacksonian myth of the garden, Southard symbolized "the politics of hope and republican enterprise, not the politics of nostalgia" (p. 156). Herein lies the essence of this tight biography of Samuel Southard. Michael Birkner has woven the public life and career of this Jeffersonian Republican turned Clay Whig into a study of the struggle of one man to excel in the early republic.

Southard was ultimately neither a success nor a failure. Schooled at Princeton, he spent five years in Virginia, where he apprenticed in the law. After returning to New Jersey in 1810, he capitalized on his father's reputation as a Republican congressman. As a leader of the Jeffersonian party, Samuel's rise to the state supreme court and then to the United States Senate was little short of meteoric. He assumed his seat in the upper house at the age of 33.

An undistinguished senator, Southard parlayed his close relationship with President James Monroe into a Navy Department cabinet post in 1823. Birkner praises Southard's six years in the department, claiming he was both professionally and personally fulfilled by his labors. Quietly moving his allegiance from John C. Calhoun to John Quincy Adams, Southard remained in the cabinet until 1829. He justified his abandonment of an earlier strong states' rights posture and the adoption of Adams's strongly nationalistic programs on the grounds that they possessed "a Jeffersonian strain."

When Jackson—a man Southard personally disliked and believed unfit for the White House—assumed the presidency in 1829, Southard rebuilt

his career. Acting as a National Republican party leader (along with Theodore Frelinghuysen), Southard was elected New Jersey attorney general, governor, and again United States senator (1833). He served for the next ten years as a loyal anti-Jacksonian and was elevated to the post of president pro tem of the Senate shortly before his death in 1842.

Birkner's Southard is a good, albeit self-seeking, man. A devoted father and long-suffering husband, he constantly struggled to obtain power and money. Maneuvering from office to office, constantly eyeing advancement, and frequently suffering disappointment, Southard seems little motivated by ideals. No political philosopher or moral crusader, he shifted from states' rights to nationalism largely because it was expedient.

This monograph will prove enlightening to those who seek a greater understanding of either New Jersey or national politics in the early nineteenth century. There are a few flaws. No personal or political intimacy seems to have existed between Southard and the other Whig leaders. This absence may, however, simply be a reflection of Southard's marginal position within the Whig hierarchy. A conclusion that placed Southard in perspective would have been helpful. These are minor points, however. Birkner has given us a solid study of a secondary figure worthy of a biography. Well researched and well written, this book does its subject justice.

JOHN M. BELOHLAVEK
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LILIAN HANDLIN. *George Bancroft: The Intellectual as Democrat*. (Cornelia and Michael Bessie.) New York: Harper and Row or Fitzhenry and Whiteside, Toronto. 1984. Pp. xvi, 415. \$24.50.

George Bancroft, America's first major scholarly historian, is no longer read for pleasure. In contrast to the volumes of his contemporaries William H. Prescott and Francis Parkman, Bancroft's *History of the United States* has long since lost its popularity. This may well be the fate of general works not centered on some dramatic historical event. Nevertheless, Bancroft continues to attract biographical and historiographical attention. He is interesting, as Lilian Handlin points out, because he is an outstanding example of a scholar in politics. The philosophical clergyman and man of letters also functioned well as a Jacksonian Democratic political leader, cabinet member, and minister to Great Britain and Prussia. Yet Bancroft would hardly be remembered in these roles had it not been for his *History*. Over the greater part of a life that spanned the nineteenth century, he conscientiously devoted himself to the

research and writing, from the sources, of a detailed multivolume account of the United States from its discovery to the adoption of the Constitution.

Handlin's biography is a superior study of Bancroft's inner life. From a thorough examination of his extensive personal correspondence, she is able to clarify many of the seeming contradictions in his career. Neither his pioneering three years abroad as a graduate student in Germany nor his plunge into radical party politics in Massachusetts wholly overcame what she sees as "the profoundly anti-individualistic and conservative approach" (p. 133) that stemmed from his Congregational upbringing and Whiggish family connections. His marriage to Sarah Dwight, even if frequently strained and unhappy, gave the young scholar financial security. All the same, Sarah's death in 1837, the year of the publication of the second volume of his *History*, apparently opened the way for Bancroft to emerge as Democratic party chieftain in New England. Even more fortuitous was his role in engineering James K. Polk's presidential nomination in 1844. Ever a realist in politics, the historian cautiously avoided his party's antislavery faction and cast his lot with the manifest destiny, Mexican War expansionists.

For all Bancroft's faith in the idea of progress and the will of the people, he was no radical reformer. He believed that America's success as a new nation was part of God's providential plan and that the collective will was superior to the idea of the freedom of the individual. "Individual conscience," he wrote in 1834, "is often the dupe of interest and often but a more honorable name for self will." "Fearing man as an individual," Handlin concludes, "Bancroft wrote his history in part to reassure himself and his audience" (p. 133).

In contrast to Russel B. Nye, Handlin stresses the conservative, less democratic side of Bancroft's philosophy and career. She is also explicit and more critical of his often unpleasant family relationships—most evident in his estrangement from the children of both his marriages. Although Nye's older biography remains an excellent guide to Bancroft the historian, Handlin's work makes him more understandable, though less appealing, as a person and politician.

ARTHUR A. EKIRCH, JR.
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RICHARD LEBEAUX. *Thoreau's Seasons*. Amherst: University of Massachusetts Press. 1984. Pp. xvi, 410.

Covering the period from the beginning of the Walden episode to Henry David Thoreau's death, this volume completes the psychobiography Richard Lebeaux began with *Young Man Thoreau* (1977). As

an indication of changed literary fashions, Lebeaux's road to *Walden* and "Civil Disobedience," unlike the famous one to Xanadu, is concerned hardly at all with literary sources but concentrates on emotional and inner life experiences. These are interpreted in expanded Freudian and Eriksonian categories and in the metaphorical application of the natural seasons to life stages.

The psychoanalysis of a historical figure from his necessarily fragmentary letters and journals is bound to be a highly speculative affair, and Lebeaux falls back on some variety of "might be" at least forty times. In a number of places, notably, in regard to the problematical friendship between Thoreau and Ralph Waldo Emerson, Lebeaux sharpens the reader's sensitivity to fascinating nuances perhaps previously inadequately appreciated, but much of the time he oversteps the line between grounded plausibility and sheer fantasy. Essentially, Lebeaux relies on three keys to the Thoreauvian arcanum: a conception of "the oedipal" that includes not only Thoreau's father but also his brother and Emerson, the notion of "ambivalence," and the seasonal analogy. In the course of an unwearied but wearying pursuit of significance in every scrap of text, these three talismen, whatever their initial suggestiveness, are worn into meaninglessness. The terms "oedipal" and "ambivalence" are mentioned over sixty times, until any explanatory function sinks into parody. In human life what, after all, is not ambivalent? And even the banal metaphor of the seasons crumbles into confusion over the long period of Thoreau's actual or subconscious dying. Where the triumvirate of the oedipal, ambivalence, and the seasons fails to inform, further appeals (about fifty) to "guilt," "anxiety," "phallic," "feces," "excrement," and even (*Gott behüte*, as Mencken would say) the "amniotic dew and ocean" shed no light.

One difficulty with this book is that the author takes Thoreau as seriously as Thoreau took himself, with the same consequent lack of perspective. Although he admits at one point that Thoreau "had a large capacity for self-deception" (p. 213), in general he treats his subject's grimly humorless spiritual dyspepsia and near-Cotton Matherish omen-mongering (always a danger for a Transcendentalist!) with portentous solemnity. Taken in bulk, as here, Thoreau demonstrates what happens when a Calvinist abandons the well-marked road to the Celestial City to live out an intuitional version of *Grace Abounding to the Chief of Sinners*. The fact is that Thoreau, a writer of imperishable prose, in his private musings, like everybody else, made many casual, ill-considered, and even foolish remarks. As a result, the true message of *Thoreau's Seasons* may well be that enthusiasts looking to Thoreau as a guide for those who would live in the spirit had

better cleave to the public texts and leave the private man alone.

FRED SOMKIN
Cornell University

JANE TURNER CENSER. *North Carolina Planters and Their Children*. Baton Rouge: Louisiana State University Press. 1984. Pp. xxv, 191. \$20.00.

Using a variety of local sources, including wills, deeds, and marriage records along with the extensive manuscript collections of family papers at Duke University and the University of North Carolina, Jane Turner Censer argues that North Carolina planters (defined as those who owned seventy or more slaves) were affectionate parents who tried to treat sons and daughters equally. In spite of the portrayal by other historians of planters as autocratic and oppressive toward their slaves, something that Censer does not deny, warm family relations, as evidenced by child-centeredness, characterized the 124 families she studied. Children of these planter families internalized parental values like the work ethic and an orientation toward achievement, which, according to the author, made the planter elite similar to the northern middle-class elite. These values encouraged the children to become substantial slaveholders themselves or to migrate to the growing Southwest with its promise of wealth and status. A final chapter in the book deals with the relationships between this planter elite and their slaves. Largely derivative, it adds nothing new to our understanding of slavery and seems an unnecessary appendage to a book devoted to planters and their children.

The author's major thesis that planters could be warm and caring parents in spite of their slaveholding reputations proves little about plantation society. Why should it be surprising to find both qualities combined in the same individuals? Nazi war criminals could in the course of a day's work send Jews to the work camps and ovens and come home as loving fathers to their own children. Leaders of organized crime are notorious for their cruelty toward associates, but they are doting and affectionate godfathers. Aside from these historical examples, common sense dictates that parents typically love their children and children their parents.

The author also uses this study of planter households as an occasion to raise broader issues that historians of the family have been debating, such as the timing of and conditions for the rise of the child-centered household and the effects of high rates of fertility and mortality on the family. Although she demonstrates an impressive command of the literature in addressing these issues, the comparisons between Europe, New England, and

the American South in different periods and under different economic circumstances contribute little toward explaining the child-centered households of the North Carolina elite. Nor does the work necessarily challenge the idea that such families were products of the Industrial Revolution, unless one assumes that the mere presence of the child-centered family in preindustrial North Carolina is sufficient to do so. More likely, wealth conditioned parental attitudes toward child mortality and underscored the instrumentality of familial solidarity. Children were undoubtedly aware of the economic benefits of affection and hardly unmindful of the economic consequences of alienation.

Although unsatisfying as an analytical study, this book has considerable merit as a description of the domestic life of plantation society. Practices of naming, nursing, and weaning of infants; the process of education and socialization of youth from the primary through the college levels; patterns of courtship, marriage, the establishment of new households, and the distribution of patrimonies—all are topics of great importance to understanding the planter ethos. This book will leave readers perhaps more anxious to hear about this side of the Old South.

CRANDALL A. SHIFFLETT

Virginia Polytechnic Institute and State University

KENNETH F. KIPLE and VIRGINIA HIMMELSTEIB KING.
Another Dimension to the Black Diaspora: Diet, Disease, and Racism. New York: Cambridge University Press. 1981. Pp. xix, 295. \$29.95.

Despite its prosaic title, this is a fascinating and stimulating book. Apart from an opening chapter on the West African background and an epilogue on the postemancipation period, Kenneth F. Kiple and Virginia Himmelsteib King focus their study on the health of North American slaves. By analyzing their historical findings in the light of modern knowledge about disease and diet, they are able to suggest some provocative answers to questions that have long concerned students of the "peculiar institution."

Kiple and King divide their work into three principal sections. The first deals with black immunities to yellow fever and malaria. These immunities—partially acquired and partially innate—protected blacks from the full impact of diseases that devastated white populations in Africa, the West Indies, and coastal North America. This fact, King and Kiple argue, must be considered by those who maintain that relatively high life expectancy statistics for slaves suggest generally good treatment. Physiological immunities, totally unrelated to treatment and living conditions, might have been the prime

reason that the life expectancy of slaves was only slightly below that of white southerners. Moreover, malaria adversely affects fertility; hence, immunities may also help explain high fertility rates among North American slaves. The irony, Kiple and King suggest, is that the immunities that protected blacks from deadly disease also made them more desirable as slaves. "If blacks had not been gifted with this disease resistance . . . many thousands or millions fewer . . . might have been uprooted from their homeland and resettled in America" (p. 67).

The second section, which the authors call the core of their book, deals with the other side of the coin: black-related disease susceptibilities. Here again, Kiple and King challenge those who emphasize the benign nature of North American slavery. Many of the slave diseases described by planters and physicians were, in the authors' analysis, diet-related. Although slave diet appeared, on the surface, to be adequate and similar to the diet of southern whites, it failed to provide the nutrients required by the black genetic heritage. The slaves' fat pork and corn diet, deficient in several basic vitamins and minerals, could increase disease susceptibilities in anyone; for blacks, genetic traits, such as darker skin pigmentation, lactose intolerance, and various hemoglobin anomalies, made these deficiencies especially dangerous. Kiple and King link diet deficiencies to such nutritional diseases as tetany, rickets, kwashiorkor, pica, and pellagra and believe that diet also made blacks more susceptible to whooping cough, pneumonia, tuberculosis, and cholera. Without the immunities to yellow fever and malaria, then, "it is difficult to escape the suspicion that . . . the collective death rate of blacks might have been far in excess of that of whites" (p. 133).

Finally, Kiple and King analyze the relationship of black disease patterns to the racist doctrines that emerged in the generation before the Civil War to justify slavery. The authors believe that southern physicians, drawing on their observations of innate black and white physiological differences, played a major role in formulating the concept of black racial inferiority. This is the least original section of the book, covering ground already plowed by such scholars as William Stanton, George Fredrickson, and Winthrop Jordan.

A brief summary fails to do justice to the range and depth of Kiple and King's work, particularly in their sections on disease immunities and susceptibilities. They have read widely and intelligently in primary sources and secondary historical works. Although this reviewer cannot judge their competence in medical and nutritional matters, their documentation in these areas also seems impressive. They are not afraid to speculate. Their effort to link symptoms described by antebellum writers to diseases not recognized until many years later is, ad-

mittedly, educated guesswork. Many of their conclusions will undoubtedly be challenged. But their ideas are continually stimulating and cannot be ignored by serious students of slavery.

ALLAN H. SPEAR
University of Minnesota

DALE BAUM. *The Civil War Party System: The Case of Massachusetts, 1848–1876*. Chapel Hill: University of North Carolina Press. 1984. Pp. xviii, 289. \$29.95.

Over the last three decades scholars have crafted a model of nineteenth-century American electoral behavior characterized by competitive party systems and by voting patterns that are fundamentally stable but are periodically disrupted by critical elections, the latter heralding a lasting voter realignment and new party system. Accompanying this interpretation of mid-century politics is an explanation of voter motivation that emphasizes religious and cultural frames of reference rather than economic concerns and local considerations rather than national issues.

In this tightly argued and statistically sophisticated monograph on Massachusetts politics from 1848 to 1876, Dale Baum has challenged certain aspects of this conventional view. According to Baum, Massachusetts experienced gradual, secular realignments of voting rather than the intense upheavals described in critical election theory. Moreover, the stream of Massachusetts politics did not flow from the Know-Nothings to the Republicans.

Not just an enlarged group of nativists, Baum's Republicans were led by a doctrinaire, tightly organized group of high-minded, antislavery moralists. Through their rhetoric and policy commitments, these Radical Republicans offered the state's voters a clear alternative to the Democrats on a changing kaleidoscope of national issues relating to antislaveryism, emancipation, and, during Reconstruction, black voting and civil rights.

The support of this popular Republican party (Lincoln carried over 70 percent of the state's vote in 1864) cannot be explained, Baum argues, by ethnocultural factors and certainly not by the pietistical and liturgical dimensions ascribed by Paul Kleppner to some midwestern states. Indeed, Baum's multivariate correlations show economic factors more consistently associated with party divisions than any other variable, although the author concludes that "the interrelations between the Massachusetts electoral system during the Civil War years and the larger social context defy a clear-cut all-encompassing interpretation of voting" (pp. 99–100).

In the early 1870s this Civil War party system gradually gave way to a more familiar version of nineteenth-century politics, which Baum calls "a political world devoted increasingly to patronage

machines and the routine business of making concessions and compromises" (p. 217). Issues became more local. Finally, the innovative, highly ideological style of Massachusetts wartime politics returned to normal as the state's intense mobilization over black issues (one that transcended religious considerations) receded.

More than most practitioners of quantitative history, Baum has interlaced his study with traditional sources, and his depiction of the famous Bird Club—that informal group of antislavery advocates—is evocative. Ultimately, though, his conclusions rest on a sophisticated statistical method (decomposition of variance) for analyzing electoral changes. Weighted percentages of the potential electorate who voted for a particular party in a given election in each county are used in a regression estimation of voter transition probabilities. The strength of this method is that it gives Baum a window onto two previously obscure groups—non-voters and abstainers. The weakness is that, like all statistical methods, it requires certain assumptions, including the presumption that certain groups act randomly in all settings.

What Baum has achieved in his monograph is twofold. First, his fresh look at mid-century Massachusetts politics will lead others to put under scrutiny the givens of nineteenth-century electoral history. Of course, not all the world was Massachusetts, though Baum does suggest (in a curiously timid double negative) that other analogies may be found in the North (p. 213). His most lasting contribution may be the attention he has given to the nonvoters and abstainers previously eclipsed in grosser methodologies based on aggregate voting data.

JEAN H. BAKER
Goucher College

WILLIAM J. COOPER, JR. *Liberty and Slavery: Southern Politics to 1860*. (Borzo.) New York: Alfred A. Knopf. 1983. Pp. vi, 309. \$6.95.

Five years after the publication of *The South and the Politics of Slavery* (1978), William J. Cooper, Jr., has returned with notable tenacity to his conviction that southern politics was informed by the single question of slavery. But this time he has expanded the chronological coverage and has cast the central thesis in terms of liberty and slavery. Readers already familiar with Cooper's earlier work will be struck by its similarities to the new work. Indeed, in places even the wording in this study echoes faithfully that found in the 1978 volume.

Nonetheless, *Liberty and Slavery* is a book of considerable merit. It is felicitously written, well organized, and intelligently argued. The author offers a convenient and even compelling synthesis of south-

ern politics across an elongated span of time. He depicts a South that from colonial days through the antebellum period occupied the contradictory position of devotion to liberty, on the one hand, and commitment to slavery, on the other.

Although one-third of the book deals with the South before 1800, Cooper's primary focus is on the nineteenth century, presumably because the theme of liberty and slavery is more apparent. The author systematically explores the impact of certain events and developments on southern politics and ideology. He argues persuasively, for example, that the financial and military exigencies of the War of 1812 pushed southerners toward nationalism, whereas afterwards the Panic of 1819 and the Missouri controversy propelled them away from nationalism. He later treats such matters as the Panic of 1837, which stimulated southern Whigs to make great exertions in the political arena, and the Wilmot Proviso, which compelled southern Democrats to embrace popular sovereignty and southern Whigs to embrace Zachary Taylor as possible solutions to the dilemmas over slavery and the territories. Cooper has much to say about the impact of the Compromise of 1850 on southern politics, revealing both its disruptive influence and, curiously, its steadying effect. Contending that southerners perceived the rise of Republicanism as the greatest threat ever, the author notes their fears that they might become the slaves of northern interests and power. In the secession crisis, claims Cooper, southern leaders urged that liberty and slavery could be saved only by an exodus from the Union.

The author's succinct survey naturally leaves some questions and problems unresolved. His portrayal of southern Whigs, for instance, engenders doubts, for it appears to be identical to his depiction of southern Democrats. Moreover, when he maintains that ethnocultural forces had little to do with political divisions in the South, one wonders what did divide the two groups. Cooper's assertion that southern Whigs in the mid-1850s died by the sword of sectional politics evokes the question: why did southern Democrats not meet a similar fate? Finally, although announcing that, from the Revolutionary era to the Civil War, "emotion was never far from the center of southern politics" (p. 42), the author seems to shove emotion off to the periphery.

Intended primarily for classroom use, the book has much to recommend it.

PAUL H. BERGERON
University of Tennessee

JAMES W. DADDYSMAN. *The Matamoros Trade: Confederate Commerce, Diplomacy, and Intrigue*. Cranbury, N.J.: Associated University Presses, for the University of Delaware, Newark. Pp. 215. \$27.50.

This study is an examination of Confederate attempts to use neutral Matamoros, located at the mouth of the Rio Grande, as an outlet for the export of cotton and as a blockade-free port for the acquisition of desperately needed Mexican and European goods. The difficulties the Confederacy encountered provide an excellent case study of the complex, interrelated problems of economics, diplomacy, and military conflict with which the Confederacy had to contend throughout its short existence.

James W. Daddysman has traced the development of Matamoros from a sleepy village to a bustling commercial center and has ably described the financial organization of the trade by Confederate and Texan financial agents, as well as the machinations of ambitious politicians, entrepreneurs, and greedy speculators on both sides of the border. Ranging beyond Matamoros itself, he has also described the commerce between Mexico and the Confederacy across the lower Rio Grande, problems between Britain and the United States over the latter's attempts to extend the blockade into Mexican waters, and the tremendous difficulties the Confederacy faced in moving goods to and from the border. Finally, Daddysman has discussed conflicts for primary authority between military and civilian authorities in Texas and between Texas officials and the Confederate government.

Daddysman convincingly refutes both the argument, advanced by Frank L. Owsley, that the Union blockade stifled the Matamoros trade and the contention that only the indifference of the Confederate government made the commerce unimportant. His thesis is that the Matamoros trade was indeed large and provided Texans with many goods unavailable in other parts of the Confederacy, but also that inadequate internal transportation, states' rights, and political conflict and disorder in Mexico, along with corruption, greed, and confusion, combined to render the trade largely ineffectual for the southern cause.

Daddysman has relied heavily on published documents and manuscript collections in the Library of Congress and at the Universities of Texas and North Carolina, and he has used a variety of British records. He has not, however, examined any Mexican (or Spanish-language) documents. The latter are essential for understanding the policies of Santiago Vidaurri, Benito Juarez, and various Mexican officials in Matamoros and the bordering Mexican states. And he has examined no secondary materials published since 1973.

Although Daddysman has considered the Matamoros trade far more thoroughly than any previous historian, there is little new or surprising concerning the broad issues. Britain's caution in challenging the blockade, Vidaurri's ambition, and Confederate confusion are all familiar stories merely

repeated here. He makes no attempt to assess either the importance of the Matamoros trade to the Confederacy as a whole or its significance for Union and Confederate relations with France, Maximilian's Mexico, or Juarez. Lacking also is any attempt to use the problems of the Matamoros trade to evaluate Confederate commercial policies and diplomacy as a whole. Daddysman has produced a careful, balanced examination that is useful only in a limited sense.

KINLEY BRAUER
University of Minnesota

GEORGE C. RABLE. *But There Was No Peace: The Role of Violence in the Politics of Reconstruction*. Athens: University of Georgia Press. 1984. Pp. xiii, 257. Cloth \$23.50, paper \$10.00.

George C. Rable is among the growing number of historians who see the American Civil War and its aftermath as a major revolutionary upheaval. His study of violence in the South between 1865 and 1877 correctly identifies conservative white resistance to Reconstruction as a counterrevolutionary movement willing to use any means necessary to eliminate Republican control of state and local government. The magnitude of paramilitary violence carried out by white Democrats was staggering, he notes, and in the end the Republican party in the North was unwilling (or unable) to preserve law, order, and free elections in the former Confederate states. Reconstruction failed, in short, because southern Democrats successfully used an un-American policy of realignment at gunpoint. Rable felicitously paraphrases von Clausewitz: "Peace became war carried on by other means" (p. 15).

This is an imaginative, well-written book that might find a niche on the reading lists of courses covering the Civil War and Reconstruction periods. Yet it does not add much new evidence to the body of research on political and racial violence in the postwar South. The counterrevolutionary character of that violence was perceptively demonstrated some years ago by Allen W. Trelease, although his study only covered the years 1865-72. Virtually all the incidents discussed by Rable have been examined by earlier scholars (not always graciously acknowledged), and his interpretations rarely differ significantly. Drawing together a general account of Reconstruction violence is, however, a useful contribution.

A second dimension of Rable's work is less successful. He attempts to use modern studies of racial and political violence and psychological theories about aggressive behavior to shed new light on Klan-like activity in the postwar South. His rendering of the various theories is simplistic to the point of

banality, however, as in the following paraphrase of John Dollard: "Periods of concentrated and prolonged frustration commonly produce aggression in humans, leading them to strike out against those they perceive to be responsible for their predicament" (p. 4). Psychologists, political scientists, and sociologists can provide valuable insights if properly understood and if used to set up hypotheses that are then tested against the relevant documentary evidence. Rable does not follow a social science approach in his research, however, and does not make effective use of the theories to which he alludes.

Trelease's earlier work eschewed psychological theories in favor of a more detailed recounting of the patterns of Reconstruction violence, spelling out the demographic, economic, and political conditions under which Klan-like organizations flourished. Rable corroborates Trelease's argument that between 1867 and 1872 there was a curvilinear pattern to the violence: white Democrats seldom resorted to violence in heavily black counties (where they might themselves have gotten shot) or in overwhelmingly white counties (where most Republicans were white Unionists). Rable shows that during the last stages of Republican control Democrats were far more willing to use paramilitary violence in black belt counties, perhaps because the prospects of federal intervention had lessened. This is a real insight, and more attention to social context might have yielded a richer understanding of why "there was no peace."

PEYTON MCCRARY
University of South Alabama

MICHAEL PERMAN. *The Road to Redemption: Southern Politics, 1869-1879*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1984. Pp. xiv, 353. \$32.00.

Michael Perman contends that the politics of the post-Civil War South has received inadequate attention from today's historians. He also believes that those who have studied Reconstruction, having brought that phenomenon to a political and chronological end, have neglected related political events after Redemption. This book attempts to remedy that deficiency.

Perman sees Reconstruction politics as consisting of fairly distinct phases. During the early years of Radical Reconstruction, both parties, or strong elements within each party, sought to take over the "center." This involved acceptance on good faith of the three Reconstruction amendments to the United States Constitution. The Democratic-Conservatives sought to win the support of Republicans, especially the support of black voters. The Republicans sought to win the support of "respectable" white conservatives.

Among Republicans this led to factionalism that pitted "regular" Republicans, nearly all of whose leaders were involved in some way with federal patronage, against the "centrists," as Perman calls them, who sustained themselves through state patronage and desired to augment the white voting strength of their party. In this interparty struggle the regulars won because they could make concessions to the black voters who were essential to electoral victory.

Within the Democratic-Conservative party, the "New Departure" faction was pitted against the Bourbons, who at this early stage opposed acknowledgment of the validity of the legal changes that had come with Reconstruction. Perman believes that the cooperation of the anti-Bourbon conservatives made it possible to end, or almost end, the depredations of the Ku Klux Klan, and these conservatives led the white South to support the Liberal Republican ticket in 1872. The campaign of 1872 made clear that the regular Republicans maintained control of the black vote. The results of this campaign seem to have been the main factor tilting the advantage within anti-Republican ranks to the Bourbons.

Perman devotes a chapter to the "old Whigs," and he concludes that the need to cater to this band of the southern political spectrum moderated the stance of both Democratic-Conservatives and Republicans. But after 1872 the center collapsed, leaving the Bourbon wing of the Democratic-Conservatives and the regular wing of the Republicans in control of their parties in those states that still had Republican administrations. Basically, this meant a black party versus a white party, and, without military support from the federal government, a black party could not maintain itself in power.

Perman's most important chapters are probably the concluding four, dealing with the politics of the Redeemers. Perman recognizes the racist justification of the Redeemer governments, but he does not find the conventional New South. In the new Bourbon state constitutions he finds more ideological meaning than most historians have found, and he attributes far greater influence to the agrarian wing of the Bourbon Democrats than do most students of the period. These last chapters are the ones that will inspire the most discussion.

This is an excellent study—well researched, well reasoned, and adequately written. The author reaches some conclusions that are certainly debatable, but his real achievement is in providing a new perspective for viewing Reconstruction and Redemption in the South.

JOE GRAY TAYLOR
McNeese State University

NORMAN B. WILKINSON. *Lammot Du Pont and the American Explosives Industry, 1850–1884*. Charlottesville: University Press of Virginia, for Eleutherian Mills-Hagley Foundation, Wilmington, Del. 1984. Pp. xii, 332. \$16.95.

John K. Winkler, in *The Dupont Dynasty* (1935), described Lammot Du Pont (1831–84) as possessing "the most original, perhaps the only really scientific mind produced in the family." Most of the attention of scholars and biographers has been directed toward the Du Ponts of the twentieth century who presided over what had become one of America's largest business enterprises. But Norman B. Wilkinson, in this splendid biography under review, has shown that Lammot Du Pont, more than any other individual, transformed the primitive Du Pont powder works of the early nineteenth century, which was organized on virtually a feudal basis, into the big business of the late nineteenth century that dominated the explosives industry and began to expand into chemical and consumer products.

As a school boy, Du Pont showed little interest in the formal, classically oriented curriculum of the day, and, even though he did better in mathematics and chemistry at the University of Pennsylvania, he emerged with only a mediocre record. With considerably more enthusiasm, he then went back into the family business, working in the laboratory for the refining and processing of gunpowder components. A consistently hard worker, he wrote that on one weekend he spent "57 out of 65 hours in the Refinery."

The powder works expanded production considerably by supplying the combatants in the Crimean War with gunpowder, and Du Pont, worried by the company's dependence on Britain for saltpeter, patented a process for producing it with different components. After the war he made an extended trip to Europe to investigate the procedures of powder producers and returned with a 104-page journal from which he devised a much-improved powder for cannon that was of great value to the federal government in the Civil War.

After 1865 he made a careful "study of work" in the now greatly expanded Du Pont plants that anticipated by two decades Frederick W. Taylor's "scientific management." In 1872, following the lead of other big business plagued by cut-throat competition, Du Pont, under the sponsorship of "Uncle Henry" who was the nominal head of the firm, created and took the lead of a "Powder Trust," which divided business among the few large companies, set prices and production, and allocated sales territories. Shortly after that, Du Pont, seeing that the great future for commercial explosives lay in dynamite, wanted the company to start producing it on a large scale. For reasons never fully explained,

Henry Du Pont refused, and young Du Pont resigned to set up his own company. He was killed in an explosion in 1884.

Although a workaholic when occasion demanded, Du Pont had a boyish zest for games, fishing, and swimming. He was always sympathetic and understanding with workmen and was admired and loved by his family and extensive kinship group.

Constructed entirely from manuscripts in the voluminous Eleutherian Mills Historical Library, this biography is well written, comprehensive, and meticulous in detail. Above all, it gives an example in microcosm of the development of a family firm from artisan workshop to big business during the Industrial Revolution.

ELISHA P. DOUGLASS
University of North Carolina

HOWARD M. FEINSTEIN. *Becoming William James*. Ithaca: Cornell University Press. 1984. Pp. 377. \$24.95.

William James is one of the most interesting figures in American intellectual history, but his complex personality has discouraged and eluded biographers. Howard M. Feinstein, a psychiatrist and historian, has finally given us a life study equal in richness to James himself.

Feinstein analyzes James by reconstructing the family that shaped him—both the intergenerational family, which descended through his father from the original William James of Albany, and the immediate family, comprised of Henry James, Sr., Mary James, and their five gifted, difficult children. The study stops in James's early thirties, when he accepted a permanent appointment at Harvard, and does not attempt an intellectual biography even of the early years. For that, Ralph Barton Perry's *The Thought and Character of William James* will remain the indispensable, if not entirely adequate, source. But Feinstein does present a recognizable likeness of James and a plausible explanation of how he acquired his unique features. In the process, we learn a great deal about work, leisure, illness, and family life in Victorian America.

William James of Albany, a Calvinist, Scots-Irish immigrant, made the family fortune in business and real estate, but his wild son Henry refused what his father most demanded—that he take up a respectable vocation and work. Henry was thereupon disinherited and had to fight in court for fourteen years to regain his portion. Henry's unresolved struggle with his father was visited on his children, particularly on William, his eldest and favorite.

On one level William's problem was his father's deep ambivalence about work. His father urged him to find a vocation, and the family finances appeared

to require it. But Henry surrounded work with so many fears and obstacles that William's adolescent indecision dragged on, forcing him deeper into doubt and despair. Hence, James's decision to teach at Harvard was the crucial turning point in his life, the beginning of adult self-confidence and regained health. Perry's emphasis on James's use of Renouvier to resolve his personal crisis is mistimed, according to Feinstein, and fails to locate the real source of his trouble or cure.

More specifically, James's problem was to decide which vocation to choose. Here again, his father's shifting preference for art, then science, then philosophy seemed to block his son's halting choices. Feinstein believes that James's first choice of painting represented his deepest personal inclination and that, when he bowed to his father's disapproval (and implied threat of suicide), James killed a crucial part of himself and began down that road of willful self-discipline that so marked his adult personality.

Beneath these vocational struggles was the psychological struggle to separate himself from his father and gain independence. William, it appears, could no more forceably push away than Henry and Mary James could let go. Through fine interpretations of drawings and texts, Feinstein reveals James's contrary impulses toward and against his father and shows how the psychological crisis of 1872, which so closely mimicked Henry's spiritual crisis, expressed the impasse to which William had come. When he went to work on his own, he finally gained the strength and distance to become, like his father, a philosopher.

Feinstein's complex argument is convincing, but it could be shaded differently. Perry's philosophical interpretation of James's crisis might well be integrated into Feinstein's view rather than discarded. James's abandonment of an artistic career might have had more to do with his own fears and less with his father's threat. Despite his excellent analyses, Feinstein explores neither the full range of ambivalent personal and cultural meanings James found in his vocational choices nor all the family tensions that bore on his development. Jean Strouse's fine study of Alice James, another biography through family reconstruction, provides essential missing information on Mary James and some sibling relationships.

Feinstein has also chosen to provide less discussion of psychological theory than he might. One would not want to alter the style of the book: it is beautifully written and elegantly produced. But a theoretical appendix would have been helpful, and the text itself could have absorbed more exploratory ventures into James's psychodynamics. Withal, this is a superb developmental biography.

DOROTHY ROSS
University of Virginia

PETER B. HALES. *Silver Cities: The Photography of American Urbanization, 1839–1915*. Philadelphia: Temple University Press. 1984. Pp. x, 315. \$47.95.

The title of this history of urban photography in the United States between 1839 and 1915 stems in part from the silver-print processes of nineteenth-century photography. They account, so Peter B. Hales argues, for the special bond between medium, cityscape, and urban concepts that characterized the output of these years. Against the background of experimentation following the introduction of the daguerreotype in 1839, he traces the relationship between picture and city through two forms: the grand-style urban photography (between 1870 and 1893) and the reform photography (between 1887 and 1915), in which new photographic techniques and different ways of looking at the city severed the traditional bonds. His discussion of the first genre focuses on the photography of the World's Columbian Exposition of 1893, that of the second on the work of Jacob Riis. In addition to Chicago and New York, the cities of Boston, Washington, D.C., Pittsburgh, Detroit, and San Francisco provide the majority of scenes.

The author deftly advances his argument through a skillful analysis of the photographs. His intimate knowledge of photography, composition, and architecture allows him to assess a picture in light of possible alternate shots. Insightful comments on the perspective of a scene as an expression of reality, intent, and message throw light on the complex relationship between the city as a place to live in and as a vision. Expanding on the significance of Riis's photographs as outlined by Alexander Alland, Ferenc M. Szasz, and Ralph F. Bogardus, Hales probes compassionately the images of the slums and their people. In sum, his well-sustained analysis of the pictures develops the theme of the city as the shared work of people open to improvements, which unites the two phases of urban photography.

In that light it is surprising that the book as cultural history rests on such a narrow base. A discussion of the mid-nineteenth-century city as the accomplishment of ordinary people would have placed into bold relief the emphasis of the grand style of the Gilded Age on buildings instead of people as another example of a society redefining its understanding of democracy. The later sections of the study, on the city's orientation toward consumerism, could have come from that rich catalogue of sources of consumption found in the extraordinary compendium of urban photographs, *King's Views of New York*, which appeared between 1896 and 1915. With the exception of the illustrations of a few so-called exposés of the wicked city, the book rarely explores the antecedents and links of urban images

among photographs, woodcuts, engravings, or lithographs. Generally, the passages providing transition, context, and perspective have stilted prose and garbled references, as if verbiage compensates for lack of penetration.

The design of the book does justice to its subject, from the double-spread title page of part of a San Francisco panorama to the last photograph, which Paul Strand labeled "Photograph—New York"—a haunting picture of a woman identified as blind by a big sign on her chest. The glossy paper and the lavish layout, which imaginatively incorporates the 201 well-reproduced illustrations into the text, rivals the magnitude and the variety of the urban scene. And the three extravagant foldouts that reproduce four large panoramas, among them the huge thirteen-plate panorama of San Francisco by Eadweard Muybridge (1878), suggest the all-encompassing and indiscriminate nature, once attributed to and claimed by photography. Yet the book brings all of its features quite handsomely together in the format of a large quarto that does not need a coffee table. The illustrations and their analysis provide a fascinating overview of nineteenth-century urban photography.

GUNTHER BARTH
University of California,
Berkeley

PERRY R. DUIS. *The Saloon: Public Drinking in Chicago and Boston, 1880–1920*. Urbana: University of Illinois Press. 1983. Pp. 380. \$24.95.

ROY ROSENZWEIG. *Eight Hours for What We Will: Workers and Leisure in an Industrial City, 1870–1920*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1983. Pp. xi, 304. \$29.95.

Social historians have been developing new methods and approaches to root out the ordinary experiences of Americans. One approach developed in recent years is the study of leisure. Perry R. Duis's study of the saloon in Chicago and Boston and Roy Rosenzweig's study of worker leisure activity are examples of it.

Duis focuses on three types of space: public space (streets, sidewalks), private space (homes, business property), and semipublic space (saloons). He finds that the number and significance of semipublic places grew during the nineteenth century, as cities became more crowded and dirty. Faced with crowded, dirty tenements and filthy streets, the city's poor poured into the saloons. Duis links the differences between Boston and Chicago drinking habits to the different structure of the industry, the dif-

ferent licensing systems, and the different alcoholic preferences of the citizens of the two cities.

Chicago's German population provided a large market for beer, and breweries proliferated. In order to limit competition, the breweries moved to gain control of the saloons, and the looseness with which licensing laws were administered enabled them to be successful. Boston's strict licensing and preference for liquor over beer led to fewer saloons, no free lunches, and a tighter market. Duis looks at the culture and functioning of the saloon. We learn about the links between corruption and the saloon and about the various campaigns to clean up, restrict, control, and eliminate it. Duis discusses the role of the saloon in the ethnic neighborhood and within the political economy of the city.

This is an interesting, well-researched, and enjoyable book. The author looks at the saloon as a social and economic institution. He tries to link it to the larger issue of the rise of semipublic space but fails in this larger purpose. The theme of the rise of semipublic space becomes lost in the details of the saloon story. Although skeptical of the argument for this theme, I was disappointed that Duis did not develop it further. I found his account of the different economic, cultural, and legal structures of Boston and Chicago fascinating and regretted that the book did not develop their comparative analysis. The author clearly loves his subject; this is the book's major strength but also its problem. In his fascination with the subject, Duis loses his theoretical ideas and ends with an almost antiquarian work.

Rosenzweig looks at both labor history and slavery for an understanding of the working class in Worcester, Massachusetts. He argues that, although Worcester's workers exhibited a notable lack of solidarity, they maintained within their ethnic communities distinct cultures that separated them from Worcester's middle class. They manifested, Rosenzweig argues, not an oppositional culture but an alternative one. The center for this alternative culture was in their leisure-time activities, particularly the celebrations on July 4, attendance at ethnic churches, and socializing at saloons. By focusing on the conflict between the workers and the city's more comfortable classes over licensing of saloons and social activities, Rosenzweig shows Worcester's workers to have had a substantially different value system and social style than the city's middle and upper class. Although Worcester's workers tended not to join unions, their separate and distinct patterns of celebrating holidays, their general rejection of legally enforced temperance, and their loyalty to ethnic gatherings and symbols seemed threatening to the middle class and were thus attacked by them. Rosenzweig shows that, despite middle- and upper-class control of the governing structure of Worcester, the city's workers managed to isolate themselves

from this cultural assault. They did so by maintaining their own institutions and modifying those forced on them by the city's leaders.

Rather than accept Worcester's lack of a strong labor movement as evidence of community consensus, Rosenzweig's analysis of the forms of leisure activity demonstrates the character of cultural conflict between Worcester's workers and employers. Worcester's working-class culture is not presented statically. The use of leisure time did change as Worcester's workers left their ethnic communities and ethnically related forms of leisure; amusement parks and movie houses replaced ethnic picnics and saloons. Rosenzweig links this development to increases in income and leisure hours for the city's workers.

Although this is a good book that offers much, it contains problems. We are told that Worcester's workers differed from others in the state in their lack of class solidarity. The significance of that difference is not explained. Rosenzweig shows us Worcester's ethnic divisions, but most industrial cities had ethnic divisions. The work deals with the issue of class but does not explain what is meant by the term. Historians have in recent years debated the issue of whether or not there can be class without class consciousness. This work could have made a contribution to that debate but did not. Although Rosenzweig demonstrates that Worcester's workers had a separate culture, I felt the argument overplayed. Rosenzweig argues that the values of mutuality and solidarity as expressed in the saloon were different from the values of the middle class. Yet examples of similar behavior could be found in middle-class, nineteenth-century fraternal organizations, or in any college fraternity.

JOHN T. CUMBLER
University of Louisville

DONNA R. GABACCIA. *From Sicily to Elizabeth Street: Housing and Social Change among Italian Immigrants, 1880-1930*. (SUNY Series in American Social History.) Albany: State University of New York Press. 1984. Pp. xxi, 174.

Scholars of the Italian-American experience have increasingly enjoyed the opportunity to carry out extensive research in the rural centers of Italian emigration. Donna R. Gabaccia joins Deno Cinel as a recent historian who derives handsome returns from a close examination of both immigrant origins and destinations.

Gabaccia examines environmental change, specifically in housing and residential space, and social behavior of the citizens of a single Sicilian town and of Sicilian immigrants living in several adjacent blocks on Manhattan Island. She justifies giving

environmental aspects central consideration by claiming that, although the environment may not make things inevitable, it does make some things impossible. For her, residential environments offer possibilities and set limits for the social and cultural life of the people living within them.

Gabaccia makes a fairly unusual distinction between social ideals and social behavior, claiming that the latter does not always or even usually conform fully to the former. This presents a methodological challenge. Most historians dealing with common folk who did not leave independent accounts of their ideals infer these ideals from their more extensively recorded behavior. For the Sicilian portion of her story, Gabaccia adopts Giuseppe Pitrè's multivolume collection of Sicilian folklore as an independent source of popular values and mores in the period prior to emigration.

Gabaccia employs a form of content analyses to extract a statement of Sicilian ideals concerning housing, family, friends, and community that she then compares with the actual residences in her village and the behavior of the residents of the various types of housing available there. She attributes deviations from the "ideals" in part to a mismatch between ideals and residential reality, although she also explores the effects of class and occupation.

Gabaccia's approach to the proverbs is a significant advance over previous uses of Pitrè's material. Her systematic analysis reveals contradictory advice and pronouncements on such important issues as family, kin, and friends and suggests caution in the casual use of such folklore as historical sources. On this side of the Atlantic, lacking a source comparable to Pitrè, she is forced back into the pattern of reading ideals from behavior.

Her arguments are carefully crafted and generally stay within the boundaries of her always interesting and often original data. Her conclusions are frequently subtle and cannot be properly summarized in the space available to me. Gabaccia is generally careful to inform her readers of the compromises forced by limitations in evidence. I would have appreciated a fuller discussion of the nature of folklore and its ties to ideals. Do proverbs reflect all aspects of a society and its culture equally, or might there be a bias toward more troublesome aspects and relationships? Also, I am not sure that there are differences as great as the text claims between her immigrants' "stripped down" culture and the "flexible tradition" Virginia Yans-McLaughlin finds among Italians in Buffalo. Such quibbles, however, do not alter the fact that Gabaccia has made a valuable contribution with this book.

JOHN W. BRIGGS
Syracuse University

FERDINANDO FASCE. *Dal mestiere alla catena: Lavoro e controllo sociale in America (1877-1920)*. (Paperbacks Università.) Genoa: Herodote; distributed by Messaggerie Italiane, Milan. 1983. Pp. x, 194. L. 16,000.

Only a few decades ago it was unthinkable that a young Genoese scholar would train himself in the intricacies of American managerial history. The study of the United States, having languished in Fascist days, had little status in Italy. This monograph reminds us how much academic fashion has changed.

Ferdinando Fasce specializes in portraying the intricacies of the shift from small manufacturing to the smoke-stack industries begun during the Gilded Age. His sources are, of course, familiar to labor historians; they are mostly written in English and have been used for many years by students of American social history. But the documentation at the end of each chapter also displays a growing number of foreign-language monographs. Although some details about immigration are by now time-worn for Americanists, such a book as this allows us to see our forebears from outside the country in which they settled.

Surprisingly, Fasce is weakest in a chapter entitled "Vagabondi, Immigrati, Ingegneri." The huge literature on Italian immigration published on this side of the ocean should have been culled more systematically. Overall organization is, however, clear. The author is at his best in describing a topic that has enchanted him: the technical contributions of Frederick Winslow Taylor, the father of modern scientific management. Today supermechanization of Turin's Fiat works represents a symbolic culmination of Italy's fascination with technological advance, despite the country's agrarian past. Hence, Fasce has understandably been intrigued by the much earlier accommodation of America to inevitable smoke-stack industries. This development brought industrial strife in its wake, and that theme also forms a major part of this book's progression.

Finally, Fasce's narrative once again illustrates how much historians today—foreign and American—prefer quantification to the probing of motives in human events. Numbers, rather than the emotions of people, dominate the text. How much more complex, indeed baffling, is the accurate portrayal of mankind's motives. A book such as this presents the historian with a superb opportunity to seek insights into the collective psychology of the very workers whose plight is at its center. But these riches will continue to elude us as long as the historians' imagination remains hobbled by slavish devotion to inert sources unrelated to emotions.

It is unfortunate that this volume is available in a language that today few Italian-Americans understand; for the book is a tribute to a new homeland of

which their predecessors would have been proud. Native-born Italian scholars are, at long last and almost too late, trying to understand America's economic and labor history.

ANDREW ROLLE
Occidental College

JONATHAN DEMBO. *Unions and Politics in Washington State, 1885-1935*. (Modern American History.) New York: Garland. 1983. Pp. 709. \$75.00.

ALBERT F. GUNNS. *Civil Liberties in Crisis: The Pacific Northwest, 1917-1940*. (Modern American History.) New York: Garland. 1983. Pp. 268. \$30.00.

The remote and sparsely populated Pacific Northwest never rates much attention in American history texts, whose chapters on the twentieth century usually include only a few words about the Oregon reformer William S. U'Ren, antiradical violence in Everett and Centralia, the Seattle General Strike, and, perhaps, Boeing's contribution to World War II or the eruption of Mount Saint Helens. The region remains the "far corner," ever on the periphery of American history. Neither of these books will change that, yet, by taking topics well discussed from a national perspective and treating them as regional case studies, both volumes illuminate the history of civil liberties and organized labor in the Pacific Northwest and, to a lesser extent, the national as a whole.

Jonathan Dembo divides his fifty-year history of unions and politics in Washington State into four chronological segments and, within each, interweaves the complex and changing economic, political, ideological, and ethnocultural patterns. The emphasis is not on working-class history as it is commonly known; rather, Dembo explores systematically and in detail institutional aspects of the American labor movement. As a consequence, many names and statistics are imbedded in Dembo's text, but few memorable personalities emerge, not even Dave Beck, who is without doubt the best-known Pacific northwesterner to leave his stamp on the national labor scene. Although Dembo spends several pages on Beck, he gives the former Teamster leader little flesh-and-blood reality apart from the ideological and organizational conflicts animating Pacific Northwest labor.

Dembo's most important contribution is to research his way through a mountain of union documents and then to offer a solid synthesis of the mass. What emerges is the history of a more or less indigenous union movement often at odds with national organizations, a condition Dembo labels "regional parochialism." At the same time, his treatment tends to downplay the importance of the

radical Industrial Workers of the World, with which the Pacific Northwest's labor movement has frequently been closely identified.

Albert Gunns, too, looks at regional radicalism. Where he makes his mark is on the history of the post-World War One era, when the forces of intolerance were largely active in state and local, not federal, government. He examines such aspects of the 1920s as the antisyndicalist laws, hostility to Asians and Communists, the Ku Klux Klan, employers' associations, and the American Civil Liberties Union (ACLU). On the civil liberties record of the New Deal, Gunns concluded that it was "far from impressive; Congress and executive agencies, in pursuit of broader objectives, probably endangered individual liberties as often as they protected them. Neglect, not always beneficial, rather than repression was the dominant characteristic of New Dealers on civil liberties issues" (p. 152). The true guardians of liberties in the 1930s, notes Gunns, were members of the ACLU and their allies working on the local level. Yet the ACLU in Washington and Oregon was itself divided on the question of defending Communists.

Both of these volumes represent revisions of doctoral dissertations done at the University of Washington. Through the medium of the Garland series of monographs on modern American history, the research of two careful and diligent scholars is readily available. I suspect, however, that Dembo's massive and detailed volume will remain primarily a reference tool, and he addresses that likelihood by providing readers an exhaustive index. Gunns writes a much shorter and livelier work. Neither author, unfortunately, provides much of a conclusion, apparently preferring to let readers sort out what they will from these basically narrative and descriptive chapters.

CARLOS A. SCHWANTES
University of Idaho

LEROY ASHBY. *Saving the Waifs: Reformers and Dependent Children, 1890-1917*. (American Civilization.) Philadelphia: Temple University Press. 1984. Pp. xii, 313. \$37.95.

LeRoy Ashby has organized his highly readable and cogently argued study of the Progressive era's child-saving movement around two overarching themes. The first is that there were a multiplicity of significantly diverging opinions regarding the strategies and priorities by which dependent children were to be redeemed. The second is that the seemingly inexorable processes of bureaucratization and professionalization eventually overwhelmed the efforts of compassionate and dedicated amateurs. No matter how contradictory this coexistence of diver-

sity and standardization may appear to be, the author strongly implies that their interaction provides much of the key for understanding not only the child-saving movement but also the entire Progressive era.

In support of this contention, Ashby presents five interesting and richly detailed case studies of diverse child-saving enterprises. The first focuses on the efforts of the Children's Home Society of Minnesota to develop national standards for the placement of dependent children in private homes while maintaining as much local autonomy in administration as possible. The second involves the efforts of the National Benevolent Association, an affiliate of the Disciples of Christ, to save the church by saving the children. Begun by women communicants seeking to build bridges among social and ethnic groups, the association evolved into a male-dominated, quasi-nativist organization dedicated to "constructing fences to protect worthy citizens from the great unwashed" (p. 102). In the third case study, Ashby details the attempts of John Gunckel and the Toledo Newsboys Association to protect the city's newsboys from exploitation by organizing them into a self-governing guild teaching the values of citizenship and entrepreneurship, an achievement that put Gunckel into direct conflict with settlement house workers who were seeking to abolish child labor.

Perhaps the most innovative approaches to the redemption of dependent children were the establishment of self-governing junior republics and farm schools, whose residents made their own laws, tried and punished violators, issued their own scrip, and sought to operate self-sustaining economies. To avoid the stigma popularly associated with orphanages, their founders pioneered home-cottages that simulated the atmosphere of family living and engaged in friendly rivalries in everything from athletics to economic productivity. Focusing on the Ford Republic in Michigan, the author demonstrates how an experiment in political and judicial democracy developed into a custodial institution whose administrators were concerned with separating the worthy from the unworthy. By contrast, Good Will Farm, with its home-cottage organization and its emphasis on such "left-over" American values as the work ethic, moral restraint, love, religious faith, and service to others in opposition to the profit motive, managed to remain largely true to the vision of its founder, G. W. Hinckley. The farm's success in preparing its residents for higher education and fulfilling lives directly challenged the National Conference on Dependent Children's contention that home placement was superior to institutionalization.

If the book has any significant weakness, it is that the author's efforts to make the case studies fit his conceptual framework sometimes seem a bit strained, especially with regard to the moderniza-

tion of the child-saving movement. That notwithstanding, he has produced a well-balanced and insightful analysis of the complexities of child-saving and provided ample documentation for his contention that, though flawed, it represented one of the finest achievements of the Progressive era.

JOHN D. BUENKER
University of Wisconsin,
Parkside

JAMES E. DEVRIES. *Race and Kinship in a Midwestern Town: The Black Experience in Monroe, Michigan, 1900-1915*. (Blacks in the New World.) Urbana: University of Illinois Press. 1984. Pp. xiii, 189. \$17.50.

In contrast to most urban black community studies, *Race and Kinship* focuses on a small midwestern town (population seven thousand in 1910), in which the known black population never exceeded thirty people. Although the book is concerned with race relations, it eschews structural discrimination for racial ideology. James E. DeVries raised two different questions: how did white residents of Monroe "learn about blacks in a setting where an Afro-American subculture was nonexistent" (p. ix) and how did these white "racist ideas constrain black life chances" (p. x)?

To answer the first question, DeVries, drawing on the work of George Fredrickson, searched for evidence of the "child-Negro" stereotype in the popular culture sources available to Monroe residents. He also extracts "beast-Negro" imagery from city reports of "Negro deviance" (p. 83). He finds whites used these stereotypes as "role models" for expected black behavior. White expectations affected individual Monroe blacks differently. Those who had the least connection to the community were most susceptible to the imposition of white imagery. For those with established family networks, "some measure of economic and of occupational success was within reach and a level of respect, recognition, and personal identity was also attainable" (p. 153). It was only through the development of extensive kinship networks, impressively reconstructed by DeVries, that blacks could gain some position and a modicum of success in a predominately white society. Full acceptance was not possible as long as one remained "black." Nevertheless, "there were as many black personal identities in Monroe as there were black people" (p. 135). Individuals and families each developed their own strategies to cope with the limits imposed by the hegemonic white society; these ranged from social protest to "passing."

This is a stimulating, innovative, and challenging study. Especially impressive are the range of sources and DeVries's use of them. The problems of the

book stem largely from the author's ambitious goals. The two questions he asks are not integrated; the answers are narrowly focused with the larger context for each remaining undeveloped. As a result, both Monroe's whites and blacks (and their relationship with each other) remain elusive. The former, despite past history and internal cleavages, are lumped together and treated as blank receptors for national stereotypes via popular culture. We get little sense of how or why whites incorporated these stereotypes into their own world view and how they benefited from them. Similarly, by focusing only on personal identity (derived entirely from white sources) and kinship, DeVries fails to develop the relationship of blacks in Monroe to each other or to neighboring black communities and Afro-American culture. Moreover, the author occasionally interprets data without supporting evidence. Finally, the study lacks several critical elements that would make it more useful to other scholars: a comparative framework, a focus on change, a discussion of methodology, and an analysis of sources.

Nevertheless, this is an exciting and valuable study. It has important methodological and conceptual insights for students of race relations, and it adds another dimension to our understanding of black urban life.

JAMES BORCHERT
Cleveland State University

SAM BASS WARNER, JR. *Province of Reason*. Cambridge: Belknap Press of Harvard University Press. 1984. Pp. viii, 302. \$20.00.

In this volume, Sam Bass Warner, Jr., a distinguished urban historian, has attempted a new style of regional history. Abandoning many conventional techniques of institutional, demographic, and quantitative history, Warner has chosen fourteen figures from the Boston area to examine a number of problems and attitudes that are of current controversy. He justifies his focus on Boston because it has the tradition of Emerson and has experienced a cycle of industrial prosperity, decay, and revival. Boston has also been in the vanguard of changing fashions and been a home of learning, especially scientific. His fourteen subjects are novelist and judge Robert Grant, Russian Jewish immigrant writer Mary Antin, radio entertainer Fred Allen, electrical engineers Charles A. Stone and Edwin S. Webster, children's book writer Laura Richards, progressive reformer and professor Emily Greene Balch, woolen mill executive William Madison Wood, leftist labor leader Fred Erwin Beal, domestic writer Louise Andrews Kent, ecologist Rachel Carson, and scientists Vannevar Bush, Laurence K. Marshall, and James B. Conant.

This book is easy to read but hard to judge as a work of professional history. It is written in a tone that at times is oddly confiding and personal, using the second person singular and the first person plural in ways that seem to assume the reader's agreement even when that agreement is in doubt. It presents little archival material or other new knowledge and frequently becomes an extended recapitulation of well-known autobiographical works. It does not challenge received historical opinion.

Instead, the book provides a gentle but firm polemic about how eminent persons have succeeded or failed in their attempts to understand their province. Those figures who come off best, such as Balch and Carson, never allowed their professional expertise to cut them off from larger community concerns. They drew instead on a romantic tradition "which stressed the relationships between the observer and the observed and found in these relationships the source of value. Those values, however, are not those of the province of reason. Our values have become those of Bush and Conant, of open competition and success. These are important half-truths, but they are far too limited and feeble to teach us about science, life, or war" (p. 257).

Judgment of such a book must therefore rest not on conventional historical standards but on whether the reader agrees with the point of view. That aside, I found myself regretting Warner's scattershot regional focus. The section on Grant, Allen, Wood, and Beal struck me as largely irrelevant to the central thrust of his arguments, as well as weak on research. The best parts of the book deal with the impact of business, science, technology, and government on human self-perception and the total environment, topics of far greater concern than third-rate novels, dead radio shows, and obscure business-labor disputes. Warner is frequently on the side of the angels, but even angels have trouble concentrating on certain topics.

The author also makes much of concrete detail and experience, yet himself falls short. His portraits tend to be snapshots, lacking memorable detail, depth, and complexity. Sustained archival work presented at greater length would have made his judgments more memorable.

ROBERT M. CRUNDEN
University of Texas

HERMANN R. MUELDER. *Missionaries and Muckrakers: The First Hundred Years of Knox College*. Urbana: University of Illinois Press. 1984. Pp. 382. \$19.95.

Hermann R. Muelder, who is college historian and professor emeritus of history at Knox College in Galesburg, Illinois, has written a delightful account of the history of the college. His story begins with

the (very American) plan to create a manual labor college in the West and to finance it by land speculation and continues to the centennial celebration of its incorporation in 1937. On the way Muelder touches on a wide variety of aspects of life at the college during these years—the religious divisions among the early trustees and the harm this did to the college, the fluctuating size of the classes, the usually tremulous state of its finances, the slowly changing curriculum, the election of the college presidents and their influence for good or ill on the institution, the hard work and devotion of the members of the small faculty, and the life of the students (though more attention is paid to their play than to their studies). We learn little about Galesburg and the relation between town and gown or about the competitive colleges nearby. What we are told about Knox merely adds to what we already know about small colleges and in no way changes that general picture. Still, the information that is related is presented gracefully and makes us feel familiar with this small denominational college (a type so common and so important in American history).

Knox seems to have had a great influence on some of its graduates and nongraduates. Edgar Lee Masters only spent one year at Knox, but three Knox professors appear in his *Spoon River Anthology*. Perhaps even more influenced by Knox—and one who carried some of the college founders' fervor for moral reform out to America—was S. S. McClure, who arrived at Knox in 1874 with fifteen cents in his pocket and only the clothes on his back. The success of *McClure's Magazine*, whose muckraking articles transformed American journalism and led to significant reform legislation, was owing not only to the somewhat erratic McClure but also to two of his classmates from 1882, John S. Phillips and Albert Brady. Since the whole class on graduation day amounted to only twenty-five, it must have been about the most influential Knox class in history.

One of the most moving parts of the book is a response, written from Knox in 1868 by an undergraduate named Barnabas Root (born Fahma Yahny in West Africa), to a query about what racial prejudice feels like. The thoughtful and literate Root clearly explains how one must battle "that blighting degrading feeling of self abasement which I see in almost every one of my race in this country" (p. 49).

There are shortcomings in this study, such as too many typographical errors, as well as a few cases of almost word-for-word repetition. The index is brief and incomplete. For those who do not know Galesburg or Knox, a map would have been helpful. There is no list of illustrations included. But these

are minor failings that only slightly mar a generally charming and well-written history.

BROOKS MATHER KELLEY
Yale University

DONALD W. CURL. *Mizner's Florida: American Resort Architecture*. (American Monograph Series.) New York: Architectural History Foundation or MIT Press, Cambridge, Mass. Pp. xii, 250. \$30.00.

A generation ago, the major practitioners, critics, and historians of architecture, still in the thrall of modernist orthodoxy, would have sneered at the idea of a serious study of the eclectic historicist Addison Mizner. From the turn of the century through the 1920s Mizner's work was fashionable in high society circles in New York and Florida. Yet, as modernism reached its ascendancy in the 1930s, 1940s, and 1950s, Mizner and others of his persuasion were increasingly viewed as something of a joke. In a popular biography of the 1950s his work was tarred as the "Bastard-Spanish-Moorish-Romanesque-Gothic-Renaissance-Bull-Market-Damn-the-Expense-Style" (p. 130).

Mizner, ironically, would not have read that as an epithet. As he asserted unabashedly in his autobiography (1932), his goal was "to make a building look traditional as though it had fought its way from a small unimportant structure to a great rambling house that took centuries of different needs and ups and downs to accomplish." "I sometimes start a house with a Romanesque corner," he confessed, "pretend that it has fallen into disrepair and been added to in the Gothic spirit, when suddenly the great wealth of the New World has poured in and the owner has added a very rich Renaissance addition" (p. 9). Such sentiments said more than Mizner and his supporters may have realized about the craving of a country as "raw" as America for a richer past than it believed it had. Yet, as modernism began to wane in the 1960s and 1970s, the "post-modern" movement of "radical eclecticism," bored with blandly repetitious "functionalism," rediscovered history and canonized Mizner's credo as the new orthodoxy.

Donald W. Curl's excellent study of Mizner's work and milieu is fodder indeed for this new avant garde, but, aside from its value as a "usable past" handbook for what is now chic, it has more lasting importance as a cultural history of a slice of American life and architecture in the early decades of the twentieth century.

A California native, descended from the earliest Anglo-American settlers, the young Mizner accompanied his parents to Guatemala where his father represented the United States as the nation's chief envoy. There, and later as a student in Spain, Mizner imbibed a taste for Hispanic culture that

would later dominate his architectural vision. After an apprenticeship in San Francisco at the turn of the century, he exploited his family's established social standing to develop among the East Coast elite a successful practice in New York and its suburbs.

While recuperating from an accident in Palm Beach, Florida, in the 1910s, Mizner was smitten by the area's felicity and decided to relocate there. From the middle 1910s to his death in the early 1930s, Mizner's clubs, villas, and public buildings deferred to the area's real and imagined Spanish past and became the shaping essence of the "Palm Beach style" of American resort architecture. His crowning achievement was the planning and designing of the neighboring city of Boca Raton, where Mizner balanced his penchant for fancy architecture for the rich with a concern for good design for the middle and working classes. In that segregated era, he also developed a model subdivision for blacks. Although much of his plan was posthumously realized, Mizner's dream for Boca Raton, like much of the Florida boom, was cut short in his own lifetime by the real estate bust of the late 1920s and the depression of the 1930s.

Curl's study of the Florida resort mentality is a welcome addition to the larger body of work on related developments in California. One wishes that he had delved more deeply and explicitly into the social ramifications of Mizner's architectural milieu in the manner, for example, of Mark Girouard's *Life in the English Country House*. Still, as a study of American resort architecture and the social and economic forces that defined it, this book is a fine contribution to our understanding of a time, place, ambience, and ethos in twentieth-century cultural history.

THOMAS S. HINES
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DOLORES HAYDEN. *Redesigning the American Dream: The Future of Housing, Work, and Family Life*. New York: W. W. Norton or George J. McLeod, Toronto. 1984. Pp. x, 270. \$17.95.

Readers of Dolores Hayden's previous books, *Seven American Utopias* (1971) and *The Grand Domestic Revolution* (1981), will know that, while each book has described, carefully and even lovingly, efforts to reform American domestic architecture in the past, each has also been searching for a housing program for the future. *Redesigning the American Dream* represents the end of that search. Here Hayden combines her descriptions of the past with an analysis of the present, making explicit the political program that had previously been only implicit. This book is a work of architectural history, conceived in the great tradition of social reconstruction.

Hayden begins with a brief account of the development of housing in the U.S. in the twentieth century, focusing on the difference between the communalized housing forms that many reformers advocated and the privatized versions that developers actually built. Referring to the single family home and the ideal nuclear family that dwells in it as "the American dream," she reviews for us the reasons why many feminists, environmentalists, and economists have begun calling it a "nightmare" instead. She proceeds to explore, using categories that she has developed herself, three nineteenth-century solutions to the housing problem, which, she argues, have conditioned the reform efforts that were actually made in the twentieth: the "haven" strategy (exemplified in the writings of Catharine Beecher), the "industrial" strategy (August Bebel), and the "neighborhood" strategy (Melusina Fay Peirce). Beecher's strategy produced the suburban tract, Bebel's the public housing project, and Peirce's the garden or greenbelt city—and each, according to Hayden, is inadequate to solve the problems that we now confront.

The third and final section of the book, aptly titled "Rethinking Public Life," begins with a particularly acute analysis of precisely those problems: the increase in mortgage rates and housing costs, which have made the single family home inaccessible; the growth of accessory, and frequently illegal, housing forms (of the five houses that line the street on which I live, three have illegal apartments in them); and the profound lack of suitable housing for the elderly, the handicapped, single parents, and single people in general. Before presenting her own solution to this housing crisis, Hayden stops to remind us that public attitudes are inextricably linked to private ones and that we will not achieve reform of residential space until we have also reconstructed public spaces.

Hayden's own approach to residential reform, which might be called the "nonsexist" strategy or perhaps "the village green revisited," is difficult for me to assess, unacquainted as I am with the nuances of housing policy or housing economics on a national scale. But, unlike many housing plans with which I am familiar, this one at least has the virtue of being based on a thorough understanding of housing history (including the alternatives that were rejected) and a keen analysis of the various factors (attitudinal, economic, sexual, demographic) that have shaped housing in the present. If profound historical insight were all that it took to make a housing program work, Hayden would be my candidate for permanent secretary of HUD.

RUTH SCHWARTZ COWAN
State University of New York,
Stony Brook

CHRISTOPHER SILVER. *Twentieth-Century Richmond: Planning, Politics, and Race*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1984. Pp. x, 342.

In this case study Christopher Silver offers his readers insights in several areas of interest to urban historians: southern urbanization, planning, urban renewal, and race relations. With the exception of the last topic, the historiography of these areas, especially for the twentieth century, is rather slender. Silver's book is therefore all the more welcome for its chronological coverage as well as for its thoughtful analysis of these important subjects.

Planning is Silver's central concern. He eschews the traditional treatment of planning that divorces it from the political context. Instead, he regards planning as a tool that served the interests of the local business elite who sought to impose its vision of metropolitan greatness on Richmond's citizens. Silver therefore sees planning as inextricably bound to a particular view of urban growth; as such, its goals and achievements cannot be adequately understood without considering the motives, successes, and failures of its supporters.

Using this perspective, Silver analyzes what is essentially a story of disappointment. In its pursuit of metropolitanism, Richmond's business elite managed to undermine the vitality of the city's inner (especially black) neighborhoods without replacing those areas with a reinvigorated commercial or industrial downtown. These businessmen failed for a variety of reasons, including their inability to dominate local politics, white flight to the suburbs, and suburban hostility to annexation. But Silver makes clear that the elite's attachment to planning was a major contributor to failure. In local politics, for example, the business elite's support of planning as a tool for growth meant it advocated an activist approach to government that Richmond's voters were only occasionally willing to endorse. When in power, the elite adopted policies and projects that alienated various constituencies who quickly abandoned the elite and its policies.

Silver's approach does have some problems. Most notably, he spends surprisingly little time analyzing the two most important political groups in his account: the "progressives" (the business elite) and the "conservatives" (advocates of the caretaker approach to local government). We learn almost nothing about either group, aside from their opposing views on the role of government. Silver rarely identifies individuals within each group, and he does not analyze their social or economic differences. Both groups remain monolithic and peculiarly obscure throughout the book.

In general, however, Silver has written a careful and valuable analysis of the political and social uses

of planning and urban renewal. This is not merely a local study of a particular city's struggles with metropolitan growth, race relations, and inner city spatial change. Drawing astutely from the historiography of all these topics, Silver always places Richmond's development in the broader context of national trends. This permits him to demonstrate how Richmond followed or diverged from those trends, thereby enriching our understanding of the complex interaction between local and national developments. Finally, his analysis provides us with a very useful account of one southern city's attempt to cope with the problems of growth in the modern era.

DAVID R. JOHNSON
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San Antonio

NORMAN D. BROWN. *Hood, Bonnet, and Little Brown Jug: Texas Politics, 1921-1928*. (Texas A&M Southwestern Studies, number 1.) College Station: Texas A&M University Press. 1984. Pp. xiv, 568. \$29.50.

In this hefty volume, Norman D. Brown describes the three fascinating phenomena of prohibition, the Ku Klux Klan, and the demagogic Fergusons in full and accurate detail. His supporting research is prodigious and includes extensive work in the materials in twenty archives and a clean sweep of relevant state and federal documents, theses and dissertations, more than forty newspapers and periodicals, and hundreds of related scholarly books. The result is one of the most carefully and fully documented state histories to appear in recent years.

Brown uses his material to good effect. The chapters on the Fergusons and Dan Moody, in particular, will interest academic and nonacademic readers alike. He describes the numerous and ingenious ways Jim ("Pa") Ferguson advanced his fortunes through control of highway construction contracts, textbook purchases, and the sale of advertising in his newspaper both to the major suppliers of materials to the state and to the dominant special interest groups. All the while, "Pa" exploited emotional issues, including evolution, the Klan, and the power of big business, to distract attention from his dealings.

Dan Moody's victory over Ferguson's wife, "Ma," in the 1926 gubernatorial election seemed to promise a better day for Texas. Young, energetic, and honest, Moody served as the state's chief executive for two consecutive terms. Yet his efforts led to few significant changes, for reasons Brown explains clearly: Moody's good intentions were not matched with sufficient skill. He was neither forceful nor guileful enough to maneuver through the shoals of wet-dry, progressive-reactionary, and Ferguson-anti-

Ferguson factionalism; he therefore failed to have a strong hand in Texas politics, let alone to wield influence in the national Democratic party. The story of his administrations is one of unfulfilled promise and lost opportunities, and Brown tells it well.

Brown also provides an interesting and useful account of the controversial presidential candidacy of Governor Alfred E. Smith, including the role of Texans in the prenomination battles and the shifting factional alignments that Smith's candidacy brought within Texas. He offers convincing support for the view that the trenchant anti-Catholicism of Methodist and Baptist leaders was more important than the more visible debate over prohibition.

For all of its obvious strengths, this study has several shortcomings. Occasionally, Brown loses the threads of his story in masses of detail. The fifth chapter, "Seeing It Through with McAdoo," for example, seems largely extraneous. More important, the author has produced a solid but old-fashioned narrative political history. It moves from primary to primary, from administration to administration, and from session to session with too little consideration of the electorate, the operation of interest groups, and the economic and social bases of political life in Texas. Although the widespread changes that occurred in Texas during the 1920s are touched on in a brief conclusion, the remarks are largely suggestive; they are not adequately tied to the lengthy text that precedes them.

These observations should not detract from the great merit of Brown's work: he has gathered an enormous quantity of information, and he has written a narrative that is informative, interesting, and clear. It now rests with other students of the period to explore more fully some of the issues he has introduced.

ROGER M. OLSEN

*University of Texas at the Permian Basin,
Odessa*

KENNETH R. MANNING. *Black Apollo of Science: The Life of Ernest Everett Just*. New York: Oxford University Press. 1983. Pp. 397. \$29.95.

Kenneth R. Manning's biography of Ernest Everett Just (1883–1941) has all the ingredients for success. It is scholarly and informative and tells a story that is nothing short of fascinating. To the casual observer, Just could readily serve as a triumphant example of the "talented tenth." A black youth raised in the South Carolina of "Pitchfork Ben" Tillman's reign and motivated and directed by an extraordinary mother, he entered the white world of the New England prep school (Kimball Union) and college (Dartmouth) and compiled outstanding academic

records. He went on to carve out a career as professor of zoology at Howard University and to earn an international reputation as a leading research scholar in the field of marine biology. Along the way were summers at the marine biology laboratory at Woods Hole, a Chicago Ph.D., work at research centers in Europe, numerous honors and foundation grants, and a list of publications filling nearly four pages of this biography (pp. 331–34).

Yet in the life story Manning relates it is tragedy rather than triumph that dominates. Apparently willing to play according to the rules of academic Jim Crow to gain entry to the scientific community, Just ultimately demanded the respect and opportunities due a scientist of his accomplishments. It was not to be. He was a "Negro scientist" to the presidents of Howard who insisted that teaching and administration of his department take precedence over research, to the grant-givers who required that his research demonstrate contributions to the advancement of his race, to his mentor and colleagues who patronized him, and to the bigots who snubbed him, insulted him, and locked him out of job opportunities at research-oriented universities. At the end of his life Just was to discover that even Europe, where he sought escape from racism during the 1930s, was not color blind.

The Just who emerges from this book is certainly worthy of our admiration and sympathy, but he is not totally without blemishes. Ambition and frustration caused him to seek financial assistance from every possible source and often to pander to the interests and prejudices of some unusual people, including Mussolini, Lady Astor, and the pretender to the Hohenzollern throne. A romantic view of Europe and its women led him to shirk responsibilities at Howard and cast off his wife and children.

Manning's work is eminently readable, and his scholarship is first-rate, most of his material being derived from unpublished manuscript sources. Detailed descriptions of Just's scientific investigations are kept to a minimum, enough to provide the reader with an understanding of his work and an appreciation of his accomplishments. Manning's tendency to identify and comment on the Jewish background of several of the characters in the book in many cases seems unnecessary and, when one considers Just's experiences, rather ironic. There are a few instances where the author offers explanations of Just's behavior without apparent supporting documentation. But, for the most part he has let the evidence speak for itself.

This biography should help gain for Just the full recognition his own generation denied him. In revealing so much about Just and the people and institutions of his world, Manning has contributed

notably to the social history of science in the United States.

FREDERICK M. BINDER
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City University of New York

MICHAEL W. HOMEL. *Down from Equality: Black Chicagoans and the Public Schools, 1920-41*. (Blacks in the New World.) Urbana: University of Illinois Press. 1984. Pp. xiii, 219. \$19.95.

Two major conclusions emerge from Michael W. Homel's analysis of Chicago's school enrollments, buildings, and budgets during the years between the two world wars: that most blacks were assigned to predominantly black schools and that these "segregated" students then received inferior treatment based on race. The first point is well documented, but the second impresses me as a triumph of previously formed convictions over the author's own evidence. There was some discrimination, to be sure, but the surprise is that there was so little.

Consider class size and school funds. In 1920 and 1925 the pupil-teacher ratio was better in the predominantly black schools than in any of the three control groups, and even in 1940, after ten years of declining white enrollments, the number of pupils per teacher in the black schools was almost identical with that in high status white schools (35.95 as compared with 35.91). There were small differences in appropriations (with the per-pupil expenditure for blacks exceeding that of high status whites by five dollars in 1925 and trailing by eleven dollars in 1940), but these stemmed primarily from seniority. With black enrollments soaring in the 1930s, most of the new teaching positions were in disproportionately black schools and were filled with beginning teachers rather than with veterans from other schools. This may have made for less effective instruction, but salary schedules that give the highest pay to teachers with a decade or more of experience have more effect on expenditures than on academic quality.

Homel concedes that most black activists supported the policy of assigning students to schools close to their homes—the policy that eventually led to "de facto segregation" as black neighborhoods expanded to encompass entire school attendance areas in Chicago. Instead of demanding that black children be dispersed throughout the city, most blacks sought to relieve overcrowding by building more neighborhood schools. They complained about the half-day sessions and temporary buildings that were commonplace in the ghetto during the depression, when new construction failed to keep pace with the burgeoning black population. But blacks knew that double shifts had also been imple-

mented in several white neighborhoods when the Caucasian population had increased substantially in the 1920s.

Homel blames the Chicago public schools for reinforcing an unjust social order instead of challenging it, but his monograph contains very little information on the process of education. Biased textbooks are mentioned, as are white teachers whose "negative feelings about black students outweighed confidence and sympathy" (p. 108). But there is almost no discussion of curricular matters or teaching methods. One puts down the book without knowing whether pupils were grouped according to ability or whether the philosophy of progressive education was influential in Chicago.

These omissions are compensated by a good discussion of the social conditions that adversely affected black education. Homel implies that, given the prevalence of discrimination, whites had no right to complain if blacks lost interest in school and drifted into idleness and crime. Yet he concedes that the quality of black education was vitiated by an epidemic of juvenile theft and violence, which are described with refreshing candor, and by sexual promiscuity, which gave blacks a rate of venereal disease that was twenty-five times greater than that among whites. For some time it has been difficult to touch on aspects of black life that reflect negatively on black people, since any criticism may cause an author to be subjected to vituperation. Homel deserves special commendation for courageously including a perceptive, albeit brief, description of the tangle of social ills that hindered black education.

RAYMOND WOLTERS
University of Delaware

DAVID TYACK *et al.* *Public Schools in Hard Times: The Great Depression and Recent Years*. Cambridge: Harvard University Press. 1984. Pp. 267. \$20.00

This is an important, interesting book. It tells the story of public schooling in the 1930s and, in the process, synthesizes a great variety of data. David Tyack *et al.* skillfully interweave discussion of what educators said about education with evidence of national demographic and financial trends and material on the politics of the New Deal. They also document the Great Depression's effects on classroom practices, which tended to reflect persistent local differences and, therefore, to remain highly diverse. The picture that emerges is one of constancy more than change. If "the maelstrom" of the depression led a few "frontier thinkers" at Teachers College to challenge educators to build a new social order, it also led school administrators to reassert traditional claims that "the equalized school" is "the only hope of the mass of our people . . . the very life

of our democratic civilization" (p. 47). To the extent that the depression fostered pedagogical innovation and greater educational opportunity for those on the bottom of the social ladder, this was a result primarily of relatively short-lived "alternative" New Deal education programs sponsored by the Work Projects Administration, the Civilian Conservation Corps, and the National Youth Administration and not a result of either "progressive" experimentation or equalization of school district resources. The authors argue, and argue convincingly, that the depression was not "a watershed" in education. The essential structures, ideology, and practices of the public schools "remained largely intact," and "the deep and abiding inequalities of class, race, and gender that structured the larger society" continued to be mirrored in the schools (p. 186).

Having provided an excellent analysis of the 1930s, accompanied by wonderful photographs, the authors conclude by contrasting the 1930s with the 1970s and 1980s. Here they are somewhat less successful. Pointing out that during the 1970s and 1980s educators again faced shortages, this time not only of money but also of students and public confidence, the authors argue that a "politics of scarcity" was a central dynamic of both eras. That is indisputable, but the contrast drawn nevertheless features similarities more than differences and, indeed, seems designed to introduce a contemporary policy position. "Public schools are institutions deeply embedded in American society," the authors argue. They are a "part of an energizing dream of social justice," and, although unable alone to overcome unemployment and poverty, they have been a "means for redistributing a rudimentary form of opportunity" and deserve public support (pp. 224-25). That may well be true. And yet, in studying the 1930s, the authors found schools more likely to perpetuate existing inequalities than to help to correct them. Because financially poor schools became poorer during the depression and the impact of school finance on school quality is emphasized, it is difficult to see how, at least during the 1930s, public schools served even a carefully delimited redistributive function. They may have become better able to promote equality of opportunity in the 1960s, and an analysis of Great Society educational programs could perhaps have shown that. Providing a full history of public schooling between 1930 and 1980, however, is not the purpose of *Public Schools in Hard Times*. Its major purpose is to analyze public schooling in the depression, and it does that fully, in lively detail, and with considerable dispassion.

ELLEN CONDLIFFE LAGEMANN
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Columbia University

ROBERT S. MCELVAINE. *The Great Depression: America, 1929-1941*. New York: Times. 1984. Pp. xiv, 402. \$19.95.

Robert S. McElvaine's survey provides a welcome new synthesis of the Great Depression. Incorporating recent scholarship, he begins with a rather sympathetic view of Hoover's struggle to cope with the early years of the economic catastrophe. He also credits Hoover with paving the way for acceptance of the New Deal, because Hoover's experiments with voluntarism had limited success in dealing with the crisis: "The New Deal owed its wide support in no small measure to the failures of Hoover's policies" (p. 69). In addition to this negative contribution, Hoover pioneered programs for relief and for aid to business and agriculture.

McElvaine recounts a familiar tale of the depression years, but he enlivens his narrative with sources that have not been commonly used before. His story is dotted with excerpts from letters of ordinary people describing their lives and their responses to the calamity that engulfed them. The most striking characteristic of the letters is the repeated praise of FDR. Despite continuing difficulties, Roosevelt commanded the affection of the masses. Even when they believed New Deal programs were not helping them, many focused blame on others and continued to express their admiration of the president.

McElvaine tries to deal with questions of values and how they changed during the 1930s. His attempts, though unconvincing, are worth noting. He argues that workers and intellectuals developed a greater respect for cooperation and that both groups were searching "for a life of community and sharing, as opposed to the acquisitive individualism of modern industrial capitalism" (p. 202).

He also attempts to get at the perceived reality of the depression experience. Using the reports of field investigators, interviews from the Federal Writer's Project, and the immense correspondence addressed to Eleanor and Franklin Roosevelt, he tries to draw a composite portrait of the responses of ordinary Americans to the Great Depression. The initial reaction to unemployment was self-criticism. Workers blamed themselves when they were unable to find work, and they were often ashamed at having to ask for assistance. By 1935, however, resentment had displaced self-blame as a characteristic response of the depression victim, and social workers noted a growing belief in entitlement among the recipients of relief. McElvaine's evidence of such fundamental attitudinal changes is not persuasive, but, by raising the issue, he points to fresh lines of inquiry that should be undertaken.

Another interesting departure for a survey of this sort is McElvaine's use of film. He studied 150 movies to uncover the ethos of the period and

found that "what was evident in the films of Capra and Ford, as well as in many other Depression-era movies, was a call for a kind of cooperative individualism that recognized individuals could achieve a degree of independence and self-respect only by cooperation" (p. 221). Both directors exalted small town virtues and condemned the amoral spirit of the marketplace.

What McElvaine fails to do is to reconcile this apparent traditionalism with the modernization of corporate and governmental structures that occurred during these years. How does the cooperative individualism exalted in films compare with the growing belief in entitlement among relief recipients? How enduring are these shifts in values? McElvaine begs the question by speaking of cycles in the country's value system. When times are bad, Americans become more cooperative or "feminine." When times are good, they become more aggressive or "masculine." McElvaine sees the thirties as a period when American values were "feminized" (p. 340). There is a quality of superficiality to these judgments. One hopes that in future works McElvaine will probe these questions more deeply. As it is, however, his book is a provocative text and should receive a broad reception.

RICHARD WEISS
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Los Angeles

JULIA KIRK BLACKWELDER. *Women of the Depression: Caste and Culture in San Antonio, 1929-1939*. (Texas A&M Southwestern Studies, number 2.) College Station: Texas A&M University Press. 1984. Pp. xviii, 279. \$22.50.

This book moves us to a new level in the history of women in the way it makes the case for understanding distinctions among women. Taking the depression experience as her starting point, Julia Kirk Blackwelder asks how economic crisis affected the family, work, and community lives of black, Hispanic, and Anglo women. Her answer is a ringing condemnation of what she calls "the caste structure of the 1930s" (p. 184).

San Antonio's population of two hundred thirty thousand entered the depression with class divisions that paralleled its ethnic and racial composition. Mexican-Americans, who made up more than one-third of the town's residents, were its poorest segment and held the least desirable and poorest paying jobs. A small community of eighteen thousand blacks was marginally better off, while one hundred thirty thousand Anglos, mostly native-born, held the better jobs, lived in the best neighborhoods, and dominated the social and political life of the city. As we might have expected, the depression hit these groups unevenly: women sometimes benefited and

sometimes suffered from the way their own cultural values intersected with the particularly difficult labor conditions of those years. For example, unmarried Anglo women, who tended to hold clerical and office jobs, managed to retain them but sometimes suffered severe wage cuts. Their married colleagues were often fired. In contrast, married Mexican-American women, who tended to work at home sewing and embroidering garments, discovered that New Deal regulations stifled the handwork industries that had provided them with employment without bringing economic benefits to their young daughters who supplemented family incomes by working outside the home at pecan-shelling. Occupational segregation thus directly benefited Anglos. Black women, who were occupationally ghettoized in domestic service, found jobs relatively easily but at continually decreasing wages.

The impacts of self-imposed cultural proscriptions, of a discriminatory and divided labor market, and of Texas's racist application of federal relief programs left these groups of women to struggle against the gender, ethnic, and racial constraints peculiar to them. No wonder it was so difficult for women to make common cause.

The power of Blackwelder's argument is enhanced by the careful use of quantitative data derived largely from the U.S. census and supplemented by written sources such as files from the Works Progress Administration, letters to Eleanor Roosevelt, and newspaper commentaries. Tables provided in the back of the book are clear and complete. Occasional muddy interpretations in the text are disturbing. For example, we are told that the divorce rate in San Antonio increased for all women, except black women (p. 27), and we are led to assume that desertion and concomitant trauma were also familiar characteristics of these difficult years. Yet we are also told that women—all women?—"understood that the family was the essential institution and economic unit that protected individual well-being" (p. 58). I longed for some clarity in the use of "class" and "culture," which sometimes seemed to be interchangeable and were never clearly defined. Blackwelder can be faulted, too, for not giving just due to the now complex literature in the field. She uses it selectively, omitting some interpretations, such as that of Ruth Milkman, that prefigured her own.

Still, this is the first solid community study that provides substance to the notion that, while some women managed to make economic progress in the depression, others, through no fault of their own, struggled against the consequences of the New Deal. The distinctions it offers should provide a model for empirical work to come.

ALICE KESSLER-HARRIS
Hofstra University

ROBERT E. SNYDER. *Cotton Crisis*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1984. Pp. xvii, 174. \$19.95.

Although Theodore Saloutos gave the 1931 cotton crisis two pages in his classic *Farmer Movements*, his recent valedictory work awards the movement a misleading half sentence. Other scholars range downward from Saloutos both in reputation and in attention given to the crisis. The aggressive Midwestern farm holiday demands are widely known, but Dixie presumably was passive.

Part of our unfamiliarity with the southern crisis lies, perhaps, in that its most flamboyant attacker was Huey Long, a "demagogue" whose power and personality, then and now, apparently require more analysis than his ideas. Robert E. Snyder deserves thanks for alleviating our ignorance. With a cotton crop maturing in 1931 that, with past carry-overs, would have amounted to twice the world's consumption of American cotton, the inadequacy of Hoover's Federal Farm Board formed a drab backdrop for Long's August bombshell: the South must outlaw the growing of cotton in 1932.

Snyder shows that millions of desperate cotton planters supported Long's proposal and that business interests, fearful of massive unemployment and of idle blacks roaming the South, were horrified. Many businessmen quickly got behind the suddenly moderate idea of holding down the crop in various ways, or, as the oil mogul who governed Texas put it, "acreage reduction and soil conservation." Opposing these partial programs, the Long forces employed remarkably modern tactics. Facing hostile print media, Long found a radio station (KWKH) that transmitted to the entire South, which allowed him to answer the editors immediately, regionally, and effectively. The medium was well suited to catchwords like "drop-a-crop" and to the promotion of biblical sabbatical comparisons. To arguments of unconstitutionality, Long furnished environmentalist answers: surely each state's police powers allowed it to strike against root rot, the boll weevil, soil depletion, and other evils that could be reduced by a cotton holiday. Everyone agreed that crop diversification was imperative, even though they feared that alternative crops faced a far shakier market than cotton's. Diversification, the Long forces argued, would therefore be aided by crops planted not for market but for subsistence.

When the cotton prohibitionists lost to the reductionists in Texas and, therefore, in the South (despite successes in Louisiana and South Carolina), did it mark the triumph of "special interests" over "the people," as Long proclaimed and Snyder often suspects? His suspicions are seldom buttressed statistically, but, if any drastic legislation in American history cries out for variance analysis of its support-

ers and opponents, surely "drop-a-crop" does. How much did the cotton holiday votes reflect personal animosity or admiration for Long, racial fears, commercial versus agricultural pressures, and traditional conservative or liberal patterns? Snyder's climetric shortcomings are slight, however, when balanced against his rescue of the cotton crisis from oblivion.

DONALD H. GRUBBS
University of the Pacific

RICHARD LOWITT. *The New Deal and the West*. (West in the Twentieth Century). Bloomington: Indiana University Press. 1984. Pp. xviii, 283. \$25.00.

One of the greatest difficulties for a scholar is to treat the history of one region in the United States. This is certainly true of the vast and diverse American West, all the more so when done in conjunction with the complicated activities of the New Deal.

In this book, Richard Lowitt attempts to provide an overview of the New Deal's work in the West. He begins by examining the role of the West in Franklin D. Roosevelt's presidential campaign in 1932, and in the penultimate chapter he deals with the political interaction between Roosevelt and the region during the rest of the 1930s. Unfortunately, the president is only occasionally on view in the intervening eleven chapters of this volume.

These eleven chapters constitute the meat of this work. They start with a survey of the misery existing in the West in 1933-34 and then sweep through the region area by area, detailing the New Deal's efforts to set things right. Lowitt makes a convincing case that the West was a poorly developed region, in which the Roosevelt administration's record would be mixed. Yet one can have little doubt that between 1932 and 1940, as Lowitt observes, "the New Deal offered the West an opportunity to transform itself" (p. 218).

Depression, along with drought, dust, and long-term plundering of natural resources, had left the West a prime candidate for rehabilitation. The federal government, according to Lowitt, seemed to be the only instrument that could supply the help the region needed. Although its efforts were mixed, the New Deal brought significant relief to the region's inhabitants. More important, during the 1930s the Roosevelt administration substantially improved the West's prospects for prosperity through conservation and developmental programs dealing with farm and range lands, forests, water supply and use, and even minerals. Also worthy of notice were the administration's activities concerning the region's Indians, recreation, and marketing of products.

Lowitt's book will be read, and deservedly so, for its contribution to the history of the West. Yet there

are serious problems with his work. Facts, figures, and ideas come tumbling forth in Lowitt's account so that too often it seems to be patchwork. Moreover, one has to guess what his concept of the West is, aside from being a geographical entity. Even the New Deal is too often defined only in terms of the programs of the Departments of Agriculture and the Interior. Lowitt's sources pose problems, too, despite the wide range of literature and archival material from which he draws. There is much pertinent material in print that he surprisingly does not cite, for example, in biographies and state historical journals. Nevertheless, one must credit Lowitt with opening the subject of the relationship between the West and the New Deal to serious exploration. It took courage to do this, given the intricacies of the topic; it will take more than courage to pursue it significantly further.

DONALD R. MCCOY
University of Kansas

JOAN HOFF-WILSON and MARJORIE LIGHTMAN, editors. *Without Precedent: The Life and Career of Eleanor Roosevelt*. Foreword by JOSEPH P. LASH. (Everywoman: Studies in History, Literature, and Culture.) Bloomington: Indiana University Press. 1984. Pp. xix, 266. \$17.50.

In addition to being the year of Orwellian comparison, 1984 marked the centenary of the birth of Anna Eleanor Roosevelt, an event many considered of much greater historical significance. The celebration of her birth witnessed a number of retrospectives devoted to her achievements as First Lady of the World and to her private and public influence. This volume of twelve essays, the product of a collaborative editorial effort between the Organization of American Historians and the Institute for Research in History, reflects the latest archival scholarship on Roosevelt. It moves debate considerably beyond the biographical work of Joseph P. Lash, Tamara K. Hareven, and James Kearney, although all the contributors are heavily indebted to Lash.

The quality of the essays is somewhat uneven, but the editors, Joan Hoff-Wilson and Marjorie Lightman, have obtained balance throughout by insisting that the focus be on Roosevelt's long public career. The essays reveal, often brilliantly, the contradictions in Roosevelt's life and underscore the tensions implicit in her public choices.

The essays proceed through Roosevelt's personal and political education and outline the development of her reform ethic. Although the contributors disagree with one another on questions such as the degree and type of Roosevelt's liberalism and the nature of her feminism, they all describe a skillful

politician, a consistent pragmatist, and an unswerving champion of individual action.

The first section, describing Roosevelt's political education, includes a spirited biographical sketch by William H. Chafe and essays on political reform by Elizabeth Isreals Perry and Susan Ware. Winifred D. Wandersee, Joanna Schneider Zangrando and Robert L. Zangrando, and Blanche Wiesen Cook contribute essays examining Roosevelt's special causes of conscience: young people, civil rights, and peace. The newest scholarship and perhaps the most interesting essays are those in the third section by Martha H. Swain, Ingrid Winther Scobie, and Richard S. Kirkendall on political friendships. These authors evaluate Roosevelt's effectiveness as intercessor and advocate within the New Deal, assess the ways in which she aided other women in politics, and hint at the political implications of her strained marriage. A final section with essays by Hareven, Abigail Q. McCarthy, and Lois Scharf considers the paradoxes of Roosevelt's position as first lady, reformer, and female.

The contributing scholars consistently avoid the classic soap-opera portrayal of Roosevelt as selfless, suffering "do-gooder." The framework also ensures that the new evidence presented will deepen the complexity of the well-known portrait and illuminate our understanding of the role women have played in American political reform. Collectively, the essays demonstrate just how much Roosevelt was constrained by class and education and make more remarkable the degree to which her public activities forced her to stretch beyond those traditional moorings. Several of the essays subtly remind us that her marriage was always the central reality of her public career. Yet one of this collection's achievements is to move debate beyond the details of domestic life and sexuality to an examination of Roosevelt's public responses to the changing dimensions of her private position, the actions those changes required, and the ways they extended and curtailed a public life "without precedent."

The considerable contributions of this volume are diminished by an editorial decision not to include footnotes for each essay, relying instead on brief bibliographical notes. The essays thereby fail to exhibit the extent of the new archival material uncovered, and questions of source and evidence go unanswered. This is unfortunate in an otherwise splendidly edited and well-written book. As the first in the new Everywoman series, one hopes this scholarly omission will be corrected in future volumes.

LINDA J. LEAR
George Washington University

PHILIPPA STRUM. *Louis D. Brandeis: Justice for the People*. Cambridge: Harvard University Press. 1984. Pp. xv, 508. \$27.50.

It has been a banner year for Louis D. Brandeis, the Progressive reformer, Zionist leader, and justice of the United States Supreme Court, who made a profound impact on American law and politics in the first half of the twentieth century. First, Lewis J. Paper's *Brandeis* appeared. And now this volume by Philippa Strum, professor of political science at the City University of New York and a vice-president of the American Civil Liberties Union. Reviewing the first book recently, I concluded that "it will be a very long time before someone writes a more concise and consistently interesting biography of 'the people's attorney' than Lewis Paper."

How time flies! Strum's effort is every bit as well researched, fascinating, and eloquent as Paper's. We now have two worthy companions to the earlier works by Alpheus T. Mason and Melvyn Urofsky, which taken together provide as rich a portrait of one man's impact on this society as we are likely to ever receive from scholars.

That impact, Strum's book makes clear, lay in Brandeis's sustained efforts over nearly half a century to give new meaning to the old values of liberty and individualism within the context of an interdependent, urban, industrial society dominated by larger and larger units of production and government. His social vision—which found expression in the battles against monopoly, in the famous "Brandeis brief," in the efforts to create a Jewish homeland in Palestine, and in his robust defense of the First Amendment—traced its roots to the classical republican ideology of Harrington, Locke, Paine, and Jefferson, all of whom believed that economic independence, or meaningful control over the means of production, was an absolute precondition for human freedom and democratic government. The classical republicans also believed that too much wealth and attention to material possessions led to "overcivilization," the erosion of moral standards, and the decline of civic virtue required as a basis for free government. Brandeis became our most passionate spokesman for classical republicanism in this century.

Strum examines the major episodes in Brandeis's life and finds that the New Englander pursued his vision with both consistency and tenacity. As Boston reformer, presidential adviser, and justice, he seldom wavered from the conviction that big business represented a dire threat to American society, that all social institutions should be tailored to the intellectual limitations of the species, and that power, whether economic or political, should remain decentralized in the states and local communities. His sponsorship of savings bank life insurance, his opposition to the Morgan-Mellen railroad empire in New England, his judicial opinions in cases such as *Liggett v. Lee* and *Erie v. Tompkins*, and his reserva-

tions about the early programs of the New Deal sprang from this vision of republicanism.

From the perspective of his numerous opponents—Wall Street bankers, business consolidators, and other collectivist zealots—Brandeis was an archaic social dreamer, whose ideas had no more utility in a world of modern industry and technology than horse-drawn buggies and flintlock muskets. But, as Strum's superb book concludes, he never abandoned the struggle against "the curse of bigness," nor did he doubt that his vision was the correct one.

Strum effectively demolishes the argument advanced by Bruce Allan Murphy in *The Brandeis/Frankfurter Connection* that Justice Brandeis's extrajudicial political efforts, especially his monetary gifts to Felix Frankfurter, fatally compromised his disinterestedness on the bench. There is a profound moral difference between a sitting justice receiving gifts (as in the case of the late Abe Fortas) and bestowing them on someone he regarded as a son. On those occasions when Frankfurter appeared before the Supreme Court on behalf of litigants, Brandeis did not participate in the case. And there is not a shred of credible evidence that he allowed his political antipathy to certain programs, such as the New Deal's Agricultural Adjustment Act, to dictate his views on their constitutionality.

MICHAEL E. PARRISH
University of California,
San Diego

MARK SILVERSTEIN. *Constitutional Faiths: Felix Frankfurter, Hugo Black, and the Process of Judicial Decision Making*. Ithaca: Cornell University Press. 1984. Pp. 234. \$24.95.

Midway between the centennials of Felix Frankfurter and Hugo Black, Mark Silverstein has given us a slim and elegant monograph on these two giants of the American bench. More than that, he has also provided a provocative and insightful perspective on the still-unsolved role of the judge in the democratic state.

His effort necessarily builds on Wallace Mendelson's *Justices Black and Frankfurter: Conflict on the Court* (1960, 1966), but it happily complements rather than overlaps the earlier book in exploring the agonistic and creative tension that separated the two modern justices and that runs back to Bentham and Blackstone before them. Moreover, Silverstein has taken full advantage of both perspective and availability of research resources that the fullness of time affords. Especially meritorious is Silverstein's magistral deployment of previously unpublished archival material in taking the measure of these two remarkable men who came out of distinctively mar-

ginal subcultures to place their stamp on the content of constitutional law.

In keeping with an initial premise that Frankfurter's and Black's perception and performance of their role as judges had been decisively shaped, perhaps at a subconsciousness level, well before either man mounted the bench, almost half the book is a biographical exploration of the roots of judicial behavior. From the point of almost concurrent accession, a controversy over due process and the Fourteenth Amendment provides a *point d'outrance* for the clashing views of the next three decades, from the flag salute cases, which served as a critical turning point in Black's attempts to displace Frankfurter from the leadership of the New Deal court, to the incorporation controversy, in which Black won the war if not the battle. The high point of that confrontation forms the climax of the book where the two variant theories of judging—the selectively elitist use of power versus the power-encasing role of law—face the problem of free expression and the needs of the democratic state.

Surely the best part of the book is the conclusion in which Silverstein develops his basic thesis of judicial power as constitutional dilemma. He refuses to resolve the dilemma and, correspondingly, refuses to award a palm to his two antagonists: "Each was ultimately guided in his task by a single-minded devotion to the Constitution and the ideal of the rule of law. In the end, perhaps, that is all we can expect of judges" (p. 219). And, indeed, that says it all.

GERALD T. DUNNE
St. Louis University

HAROLD LEE. *Roswell Garst: A Biography*. (Henry A. Wallace Series on Agricultural History and Rural Studies.) Ames: Iowa State University Press. 1984. Pp. xv, 310. \$12.95.

Roswell Garst was unknown to most Americans before September 23, 1959, when Nikita Khrushchev visited his farm near Coon Rapids, Iowa. The evening television news showed Garst explaining some of his farming operations to Khrushchev while being followed by a host of annoying reporters. Why was the Russian leader walking around farm lots visiting with Garst? The answer was simple. Khrushchev believed that Garst was one of America's most progressive farmers and that the Russians could learn much from him.

Although Garst was known only in limited circles before 1959, he had been in the vanguard of America's agricultural revolution in the 1930s and 1940s. Born in 1898 at Coon Rapids, he began farming a 200-acre farm in 1921. In 1926, however, he moved to Des Moines to engage in the real estate

business. There he struck up a friendship with Henry A. Wallace and became intensely interested in hybrid corn. Returning to his farm in 1930, Garst made arrangements with Wallace to grow and sell hybrid seed. The next year, with the help of Charley Thomas, he organized Garst and Thomas Hi-Bred Corn Company. During the next few years, Garst pushed the sale and planting of hybrid seed corn with evangelistic urgency. Hybrid seed, Garst argued, would greatly increase farm productivity and efficiency in the Corn Belt. More farm production with less labor was his battle cry.

Partly as a result of Garst's efforts, the planting of hybrid corn spread rapidly throughout the Midwest. Next he turned to the question of chemical fertilizers. At a time when very little chemical fertilizer was being used in the Corn Belt, he insisted that farmers should apply heavy amounts of nitrogen. By the early 1940s, Garst was pushing a nitrogen program with the same intensity and enthusiasm that he did hybrid corn a decade earlier. In some cases his own farm experiments were ahead of those carried on at Iowa State College.

In 1953 Garst began to think about using American farm surpluses to ease East-West tensions. This coincided with the rise of Khrushchev and the Soviet Union's need for improved agricultural production. After meeting a Russian delegation in the United States in 1955, Garst was invited to the Soviet Union in September. He met with Khrushchev and sold the Russians some hybrid seed. Subsequently, Russian delegations visited the Garst farm, and Garst returned to Russia on several later occasions. There is little doubt that Garst had a significant impact on Russian agriculture, but he was unable to reduce international tensions as he had hoped.

Garst was an uncommon common man. He was a curious, enthusiastic, intelligent, dynamic super-salesman of ideas for agricultural modernization. He personified many of the qualities that have made the nation great. He was innovative, unafraid to take chances, and strongly determined to implement his ideas; he had great compassion for people.

Harold Lee, Garst's son-in-law, has written a fascinating biography of this important American. It is based on the voluminous Garst correspondence as well as on other sources. Despite the family relationship, Lee portrays Garst with warts and all. Although the book's main theme deals with Garst's work as an agricultural leader, his personal and family life are woven skillfully throughout the book.

This is the second volume in the Henry A. Wallace Series on Agricultural History and Rural Studies published by the Iowa State University Press. It is a very strong addition to the series.

GILBERT C. FITE
University of Georgia

ROGER E. BILSTEIN. *Flight Patterns: Trends of Aeronautical Development in the United States, 1918-1929*. Athens: University of Georgia Press. 1983. Pp. xi, 236. \$18.50.

This small book is an excellent summary of the emergence of the airplane in American society between the end of World War I and the beginning of the Great Depression in 1929. Roger E. Bilstein is interested not only in the usual technological developments but also in the economic, social, and political ramifications of air vehicles and aerial flight. Most writers on aviation have concluded that army and navy expenditures for aircraft kept aircraft manufacturers solvent during the lean post-World War I years and that Charles A. Lindbergh's flight to Paris in May 1927 marked the takeoff of commercial aviation. But Bilstein shows that Lindbergh's feat was more in the nature of a catalyst for aviation growth patterns already underway before 1927. Bilstein also believes that the earlier focus on the "Lindbergh boom" as well as on the "indomitable" Douglas DC-3 transport plane has tended to obscure the aviation trends prevailing in the 1920s that were both basic to Lindbergh's success and contributed to the unusually rapid progress of aviation in the 1930s.

In addition to advancing this new thesis, Bilstein provides short, sometimes merely chronological, accounts of air mail, commercial flight beginnings, air express and air cargo, barnstorming, private business flying, crop-dusting, seeding and forestry, uses in urban and regional planning, and aerial photography, all of which began in the 1920s. He treats the development of aeronautical research and development, emphasizing the role of the National Committee for Aeronautics. He considers new thinking about the physiology of flight, the science of meteorology, and the effects on manufacturing and finance. The last chapter is a pioneering essay on the effect of aviation on American cultural symbolism and imagery. How many people know, for example, that Thomas Hart Benton was one of the first recognized artists to use aeronautical symbolism—this in "Bootleggers" (1927) where rumrunners are using air transport?

Because of the many subjects addressed in this sweeping survey, Bilstein is not allowed enough Linotype lines for any one of them. This will probably reduce the pleasure of buffs or students seeking content. There are next to no errors, but Josephus Daniels (p. 23) was secretary of the navy, not war, and Colonel Edgar S. Gorrell (p. 8) did talk about strategic bombing but later apostasized, accepting conventional army views. But nothing said here should detract from the judgment that Bilstein has made a fine contribution to aviation history that is a credit to him and to the Smithsonian's National Air

and Space Museum where, as visiting scholar in aerospace history, he got time to perfect his final narrative.

ROBERT FRANK FUTRELL

*Center for Aerospace Doctrine, Research and Education
Air University*

JOHN F. SHINER. *Foulois and the U.S. Army Air Corps, 1931-1935*. (General Histories.) Washington, D.C.: Office of Air Force History. 1983. Pp. xv, 346. \$13.00.

John F. Shiner, now deputy director of the Office of Air Force History, is the author of this important study of the U.S. Army Air Corps in its transition from being a tool of corps commanders to being an independent force under the GHQ Air Force, still a division of the army. Its role changed from being amorphous and ill-defined to being specifically connected with repelling invasion. It is a complex political story centered on the Office of the Chief of the Air Corps, then headed by Major-General Benjamin Foulois. Originally a mechanic, Foulois had come up through the ranks and loved to fly. Some of his difficulties stemmed from the fact that he was not a West Pointer and some from his being out of the office flying around the country visiting his units. Other problems came from the seniority rules under which his corps worked, which placed his officers at the bottom of the promotional heap; still other problems from economic constraints. But, as Shiner shows well, his own desire to vindicate air power led him into constant testimony against the General Staff, much to their irritation. In the end he found himself charged by the House Military Affairs Committee with various crimes. It is doubtful that he was more than technically guilty owing to the nature of the laws, but he spent his last year in office fighting to save his reputation and was far less outspoken than he had been earlier. Nevertheless, out of his struggles came the Baker Board and the Civil Aeronautics Commission, both of which were spurred on by the abysmal performance of the unprepared Air Corps in the air-mail fiasco. The results were that the GHQ Air Force, to which the Army General Staff had paid lip-service for nine years, had to be implemented, promotions had to be opened, funds had to be released for additional personnel and procurement, and the Air Corps was allowed to begin to order modern aircraft.

Shiner dwells on the problems as seen at the top and in the documents. The story has recently been told in more lively human fashion by DeWitt Copp in *A Few Great Captains* (1980). Shiner touches briefly on the technological revolution that was taking place in aviation at the time, but, as his bibliography makes clear, this is outside his purview. And yet it

was the technological revolution both in the United States and abroad that was one of the forces beginning to put pressure on air force budgets, as he points out when he notes that the procurement of new machines by the mid-1930s was failing to keep up with wastage. An examination of the bibliography also shows why this is a much-needed book and why it is to be hoped that a sequel is in train. American aviation history is an absurdly neglected field considering aviation's important role in the twentieth century.

ROBIN HIGHAM
Kansas State University

KENNY A. FRANKS. *Citizen Soldiers: Oklahoma's National Guard*. (Oklahoma Horizons Series.) Norman: University of Oklahoma Press. 1984. Pp. xiii, 234. \$24.95.

This is one of the better histories of the National Guard of an individual state. Kenny A. Franks has studied responsible sources and compiled a bibliography of them at the end of the book. Included in the text are fine maps and scores of pictures. The final chapter on the Total Force, a term introduced in 1970 by Secretary of Defense Melvin Laird, is especially useful. After 1970, the National Guard ceased to be a part of the strategic reserve and became instead a component of the force-in-being, presumably ready at short notice. The National Guard Association had worked for this status for a century.

Like most histories about the militia, or National Guard, of individual states, this one focuses on participation in the country's wars. Of the 192 pages, 70 are standard combat narrative, using "bloody," "merciless," "bitter," and similar words to describe the fighting. General Patton spoke of the Forty-fifth Division (composed of the national guards of Oklahoma, Arizona, Colorado, and New Mexico) as being baptized in blood in Sicily. He savored such figures of speech, but the narrative conveys little of the feeling of what it was like to be so baptized. One can examine war in numerous ways, and muting the horror of it, used here, is one of the most common. The courage and determination of the troops come through strongly, but little else.

Franks takes only 48 pages to reach the year 1940; most of the combat narrative describes action during World War II. In contrast to the 70 pages devoted to combat, only 20 pages deal with the use of the Oklahoma Guard to maintain law and order within that state and give aid in times of disaster. This is an imbalance in the study, because the state uses of organized citizen soldiers are as important as the uses by the nation in wartime. The National

Guard is among the oldest American institutions and has had an important impact in shaping the nation in peacetime, which deserves more emphasis than it receives here.

The record of the Forty-fifth Infantry Division in World War II was distinguished. In 511 combat-days it lost 2,540 officers and 60,023 enlisted men, killed, wounded, or missing (these are the author's figures). It is not clear how many participants were Oklahomans since it had been brought up to strength by heavy infusions of draftees, had lost roughly four times as many men as its table of organization called for, and at the start was composed of the national guards of Arizona, Colorado, New Mexico, and Oklahoma. Nevertheless, the author treats the Thunderbirds (as the Forty-fifth is called) mainly as a part of the history of Oklahoma. When the war was over, the colors and honors did return to that state; maybe where the campaign streamers are is where the soul of a division is. During the Korean action, there were two Forty-fifth Infantry Divisions, one in Korea and the other in Oklahoma; the two were later merged back in Oklahoma.

Besides the pivotal five chapters on World War II, there are chapters on the use of the Oklahoma National Guard on the Mexican border, in World War I, during the Cold War, and in Korea. The other five chapters cover territorial days (back to the 1880s), organization of the Forty-fifth Division interwar years, the Air National Guard, and the Total Force.

Of its kind this is a competent history, but I think the kind needs to undergo some basic changes.

JOHN K. MAHON
University of Florida

RICHARD GID POWERS. *G-Men: Hoover's FBI in American Popular Culture*. Carbondale: Southern Illinois University Press. 1983. Pp. xix, 356. \$20.00.

In this book, Richard Gid Powers examines the evolving public image of J. Edgar Hoover's FBI, arguing that it was "popular culture, as a reflection of the political subconscious, that defined the role Hoover was to play in American life" (p. xii). Using a classic American Studies approach, Powers emphasizes myth and symbol analysis as a way into the "collective hopes and fears of the mass society." He is partially successful in explaining the ups and downs of the FBI's public reputation from the Great Depression through Watergate. The image making began in 1933, according to Powers, with the creation of the modern FBI as part of a larger campaign by Attorney General Homer Cummings to make law enforcement and the New Deal-inspired "war

on crime" a national movement dependent on public support.

Powers argues that Cummings's original model of the efficient and scientific (but faceless and anonymous) FBI agent became subverted by the "G-Man" formula in American popular culture. The cycle of popular films begun by "G-Men" (1935) adapted the older action detective hero, who appealed to projective fantasies of individual power. Cummings's and Hoover's attempts to project an image of their agents as essentially bureaucratic heroes devoted to teamwork were no match for James Cagney. Powers casts his story as basically "a war between the G-Man formula and the FBI formula for what was the soul of Hoover's FBI—its public image" (p. 112). The bulk of the book examines this struggle as played out among the various portrayals of the FBI in pulps, comic strips, books, radio shows, and television series.

Most significant, Powers finds that by World War II Hoover and the bureau's public relations machine had thoroughly internalized the G-Man formula, in which the director was "Public Hero Number One," the ultimate action detective. But after the war Hoover sought to raise the image of the FBI from mere law enforcement leadership to the defense of traditional America and patriotic values. In so doing, writes Powers, he "loosened his grip on the pop culture formula that had made him and his men culture heroes" (p. 229). With the decline of cultural and political consensus brought on by the civil rights struggle, the New Left, and the sixties counterculture, the bureau's image was turned on its head: Watergate and new scholarship revealed the sordid underside of black bag jobs, illegal wiretaps, character assassination, and political harassment directed from Hoover's office.

Powers is most interesting and persuasive in the earlier chapters on the thirties, particularly in detailing the myriad strategies by which Hoover subordinated law enforcement to the creation of "one of the greatest publicity-generating machines the country had ever seen" (p. 95). This book is quite suggestive in discussing the ways in which commercial popular culture has proved to be a sort of historical joker in the deck of twentieth-century politics. But Powers's case is weakened and his overall presentation limited by schematic American Studies terminology that does little to further historical explanation. The author overindulges in phrases such as "national morale," "public mood," and "America was ready for rituals of national unity" (p. 136). Lurking behind much of the provocative myth and symbol analysis lies stilted sociology and reductionist psychology of the worst kind: "The Left may have moved from a neutral attitude towards the bureau to one of reflexive hostility as a psychological defense mechanism" (p. 278).

Powers points out that, even at the chilliest moments of the domestic Cold War, vocal dissenters from the FBI myth (such as Max Lowenthal and Fred J. Cook) publicly documented the abuses of the bureau. The politically motivated harassment of many thousands of Americans was more than a myth or symbol—the victims and their families certainly knew that. Powers's analysis tends to flatten out and de-politicize American responses to the FBI under the catch-all of "popular culture," a term he uses anthropomorphically, as if it were an independent historical actor. Powers's contribution is to make us think more carefully about the interplay between commercial popular culture and the government's public relations efforts. Perhaps greater use of archival and unpublished materials might have narrowed the gap between myth and symbol analysis and the actual historical development of the FBI's image-making power.

DANIEL J. CZITROM
Mount Holyoke College

WILLIAM S. BORDEN. *The Pacific Alliance: United States Foreign Economic Policy and Japanese Trade Recovery, 1947–1955*. Madison: University of Wisconsin Press. 1984. Pp. xiii, 320. \$25.00.

A recent trend in the historiography of U.S.-Japanese relations has been to stress continuity in the two countries' compatibility and interdependence rather than friction and rivalry. They are depicted as having more often than not been cooperative partners in a search for world order in which their shared interests would be promoted. This book is the latest addition to that school of interpretation.

William S. Borden reflects the thinking and methodologies of his mentors, particularly John Dower and Thomas McCormick. Like them, Borden stresses economic multilateralism as a key to American foreign policy and develops the theme of reintegrationism to account for the postwar rapprochement between Tokyo and Washington. These ideas have been presented by other historians, so that what the book does is to confirm existing formulations rather than offer a novel interpretation.

The author, however, makes a distinct contribution in relating postwar U.S.-Japanese affairs to the larger framework of American strategy in Asia. Others, notably Michael Schaller, have done so, but Borden's is the first systematic analysis of American economic policy in the region. He argues that to implement its objective of a multilaterally linked world, which was considered essential for America's own prosperity, the United States government early recognized the need to encourage and assist Japanese reindustrialization and reparticipation in inter-

national trade. Southeast Asia would be particularly important in this respect. "Since the United States and Europe tended to monopolize trade in Latin America and Africa respectively," Dean Acheson said, "Japan would have to compete successfully in Southeast Asia in order to prosper" (p. 120).

This was the very region into which Japan had sought to expand by force and to which the European powers were anxious to return to regain their control over resources and markets. The American policy of promoting Japanese reentry into the region had the effect of opposing both European colonialism and indigenous nationalism calling for economic development through industrialization. The latter was a genuine threat to multilateralism, but American officials argued that Southeast Asia's prosperity need not depend on industrial development but could be achieved through the region's integration into the world economy—through Japanese trade.

All this sounded fine on paper, but before the Korean War, Borden argues, Japan lacked sufficient funds to undertake reindustrialization. The war changed this through America's massive procurement of Japanese goods, particularly military arms. By the end of the war, the United States had succeeded in laying the groundwork for Japan's emergence as a major economic power. "Southeast Asian integration and military procurements" (p. 192) were the secret of this success, but such a strategy inexorably led to American military interventionism.

The stress throughout the book is on economic themes. The author does not neglect security and ideological issues, but such macrolevel factors are rejected as insufficient, for they do not really explain why, soon after the war, the United States risked resentment by China, Britain, the Philippines, and others by restoring a relationship of interdependence with Japan. Borden's analysis amounts to establishing a connection between America's business interests oriented toward multilateral trade and Japan's postwar leaders similarly intent upon reviving the country's position in the world. Although by no means original and despite the author's use of terms such as "the old order" and "militarist" to describe Japanese leaders, the thesis gains credence in this well-researched monograph.

AKIRA IRIYE
University of Chicago

ROBERT J. DONOVAN. *Tumultuous Years: The Presidency of Harry S. Truman, 1949–1953*. New York: W. W. Norton. 1982. Pp. 444. \$9.95.

Although 1984 is the hundredth anniversary of Harry S. Truman's birth, there still is no generally

respected academic history of the Truman administration, which remains a scholarly battleground between Cold War traditionalists, progressive "revisionists," and neo-Cold War "postrevisionists," who debate the merits of an administration that established the political consensus that has shaped recent U.S. and world history. Robert J. Donovan, a veteran Washington journalist, has now published the second volume of his history of the Truman years, chronicling the administration's trials in what might be called the "High Cold War period" of the early postwar era (the formation of NATO, the development of the nuclear arms race with the USSR, the Korean War, and the political climate and purges known popularly as "McCarthyism"). Although he uses extensive manuscript sources and interviews, along with select secondary sources, Donovan has written an Associated Press history of the second Truman administration, transforming private sources into the language of front-page newspaper copy and history into a barrage of facts and anecdotes flooding a suddenly imperial Washington from the capitals of Europe and Asia.

Donovan's work is too frenetically detailed to be effective popular history and too lacking in either a developed analytical framework or attention to the extensive historiographical conflicts on the topics he treats to be considered an important contribution to academic history. It has value, however, both as an often colorful reference work on the Truman years and, to a far lesser extent, as an expression of the influence of the conflicts among academic historians on the shaping of popular conceptions of controversial questions in recent history. In a work that is essentially an uncritical appraisal of the Truman presidency, Donovan shows in a few surprising moments the influence of the critical progressive scholarship of the Truman years, which scholars of the conventional wisdom call "revisionism." On McCarthyism, for example, Donovan notes that "there was enough blame for everyone, including Truman, for the soil that brought forth McCarthy and enabled him to flourish well into the Eisenhower years" (p. 163). Also, U.S. policies of support for Chiang Kai-shek bring forth this insight: "Chiang's regime was cruel, corrupt, reactionary, inept, undemocratic, and unpopular. It was a dying order" (p. 67).

Yet Donovan, writing a jam-packed narrative that is half old-fashioned "court history" and half journalism with manuscript citations, really fails to digest his materials, much less develop his study. His uncritical Cold War framework of anticommunism and anti-Sovietism, which still informs the great bulk of both academic and popular histories of the Truman period, makes it impossible for him to deal with either the internal changes in American politics and culture wrought by the Cold Warriors or the intent and the practical effects of postwar U.S.

foreign policy. That framework, on which the Truman policies were built, served as a demonology for Truman and his successors, as it does for Reagan today. For many academic and popular historians, it still acts as a defense of provincialism in both politics and scholarship, a sort of Truman Doctrine against taking seriously the outlooks of either academic "revisionists" in the United States or diverse critics of U.S. policy in the international community. As such, this framework produces both academic and popular works that, like *The Tumultuous Years*, literally forget nothing and learn nothing from history.

NORMAN MARKOWITZ
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ROGER BELL. *Last among Equals: Hawaiian Statehood and American Politics*. Honolulu: University of Hawaii Press. 1984. Pp. x, 377. \$24.95.

The story of Hawaiian statehood merits the attention of historians both because it is typical and because it is unique. Like most other states, Hawaii passed through territorial status on its way to becoming a state. But because of the recentness of its experience, the benefits and drawbacks of the process of achieving statehood can be discussed with greater immediacy for today's reader. The uniqueness of the story is owing not only to Hawaii's physical separation from the mainland but also to its multiracial composition. This cultural distinctiveness, which made Hawaii's transition to statehood more difficult than that of most other states, gives the story its special historical significance.

In this book Roger Bell recounts the formal process of achieving statehood, including full reports of the recurrent debates in Congress and the activities of statehood advocates in Hawaii. He sometimes provides needless and redundant details on these matters, as in the analysis of congressional strategies and debates between 1949 and 1956. Although these discussions provide excellent case studies of how legislative strategy affects policy outcome, they seem more appropriate to a study of congressional process than to the present volume. Bell's equal thoroughness in treating social developments and party politics in Hawaii itself, however, is truly instructive. A particularly valuable chapter, "Postwar Hawaii: An Americanized Community?", focuses on the acculturation of Hawaii's multiple ethnic groups after World War II and on the relationships between ethnicity, economic development in the islands, and the struggle for statehood.

By the 1950s, Bell asserts, "acculturation was well developed if less pervasive than in mainland communities," but structural assimilation (by which he apparently means economic integration) of the various groups was "limited and uneven" (p. 118). And,

since class and ethnicity tended to be related in predictable ways (for example, Japanese-Americans were the group most closely associated with radical unionism in the islands), conservative foes of statehood could disguise their racist objections as fears concerning Asian communism. But, even during the height of the anticommunist crusade in Washington, "the loyalty, acculturation, and assimilation of Hawaii's multiracial community were not the stumbling blocks to statehood. Multiracialism itself was the issue" (p. 294). To support his thesis that racism outweighed all other considerations in the minds of politicians who opposed Hawaiian statehood, Bell examines the convergence of debates and roll call votes in Congress on the issues of statehood for the islands and civil rights generally. Although he sometimes seems to dismiss too casually the deeply held motives of anticommunism and economic self-interest, his thesis is generally convincing.

Throughout his study Bell relies heavily on such useful sources as the papers of Joseph Farrington and the Hawaii Statehood Commission and on the rich contents of the John A. Burns Oral History Project. Together with his research in the various presidential libraries, these sources provide the basis for a well-informed and interesting discussion of the interplay of territorial or state and national politics, something rarely achieved in a state history. Although the book contains more exhaustive detail than any but a specialist on Hawaii will want, it nevertheless transcends parochialism by tying the question of Hawaiian statehood to issues of major national importance.

GARY W. REICHARD
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DAVID R. JOHNSON *et al.*, editors. *The Politics of San Antonio: Community, Progress, and Power*. Lincoln: University of Nebraska Press. 1983. Pp. xi, 248. Cloth \$24.50, paper \$10.95.

Geraldine Ferraro proved a popular choice as Walter Mondale's vice-presidential running mate, but probably not with the authors of this book. Henry Cisneros might have done more for sales, although his 1981 election as mayor of San Antonio is not a prominent feature of the book's analysis of power and progress. This may represent a considered judgment by the authors, or it may be that Cisneros's victory came after the articles were substantially completed.

The Politics of San Antonio is a joint effort by fourteen historians, political scientists, geographers, demographers, and sociologists to provide a multidisciplinary analysis of the city's current political status. We hear briefly of San Antonio's political past—that it is economically typical of metropolitan

Sun Belt and Texas cities and that Mexican-Americans may have better chances for occupational mobility in San Antonio than elsewhere in the United States. These improved chances are, however, inferior to those of San Antonio's "Anglos."

After this multidisciplinary introduction, the book concentrates on San Antonio's unusual recent political history. The numerous authors are in substantial agreement that the reform movement to replace a corrupt commission system of government with a council-manager structure in the early 1950s led to domination of the political process for twenty years by a nonpartisan Good Government League through which the city's elite exercised power. The authors also agree that the dominance of this elite broke down in the 1970s, owing in part to a rise in the city's Mexican-American population to a majority and in part to changes brought on by the 1975 amendments to the federal Voting Rights Act, including reform of the city council from at-large to district representation. The chief question addressed is whether San Antonio politics will become increasingly conflictual and "democratic" in the 1980s, or whether a reconstituted elite will regain control.

The evaluative chapters anticipate more democracy, although there is reason to suspect that the elite could make a cozy alliance with Henry Cisneros, helping him to achieve his purposes and theirs at the same time. Overall, the book is a disappointment. Much of the volume attempts to do no more than use San Antonio to document the supposed ease with which a white economic and social elite can use city politics to control the destinies of minorities. Although this may have been true of San Antonio politics before 1971 (a period that is of only secondary concern to the authors), it does not do justice to the fascinating changes since then.

None of the authors is sufficiently skeptical about events and their meaning. No author properly investigated the social make-up of the elite in its heyday or in its recent eclipse. The principal underdogs are taken for granted as well. Primarily they are referred to as "Mexican-Americans." The term "Chicano" appears often in some articles and not at all in others. Which San Antonians consider themselves Chicanos and which Mexican-Americans—and with what intention—remains a mystery. One chapter reports that voter turn-out of registered Mexican-Americans in Cisneros's 1981 mayoral election was weaker than that of "Anglos," a substantial proportion of whom voted for Cisneros. This seems to cast a shadow on most of the book's analysis of the current situation, but none of the authors attempts to explain its significance.

Many chapters are organized to test hypotheses, using San Antonio as a source of data, as if they were articles in a political science journal. Hypothe-

sis testing is valuable for refining sophisticated theories of political action, but it cannot explain the politics of a single city. This reader finished the book longing for some old-fashioned unmasking of the cardboard characters and their ethnic melodramas.

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CANADA

BARRY M. GOUGH. *Gunboat Frontier: British Maritime Authority and Northwest Coast Indians, 1846-90*. (University of British Columbia Press Pacific Maritime Studies, number 4.) Vancouver: University of British Columbia. 1984. Pp. xvii, 287. \$22.95.

Two Canadian historians have lately been giving monographic scrutiny to the Pacific coast of British North America. In two volumes, Glynn Barratt examined the Russian naval presence. In *Gunboat Frontier*, which completes a trilogy on British naval activity, Barry M. Gough is concerned with the involvement of policy and practice of Anglo-Indian relations. Gough and Barratt explore a little-known dimension of the history of the area. Their path-breaking studies suggest a rewarding perspective from which to examine Russian, British, Canadian, and American interaction in the region. Fur traders, missionaries, pioneer settlers, and assorted government officials—the conventional list of those who were active in the confrontation of cultures on the frontier—were land based. In his work Gough plumbs the naval records, a neglected primary source, to determine the Royal Navy's role as an instrument of British policy.

The second greatest density of Indian population in North America was along the coastal areas of British Columbia. A salubrious climate, the rich abundance of fish and forest life, and the relative geographic isolation fostered the development of a wide diversity of maritime, autonomous, village-oriented cultures. The Haida, Tlingit, and Tsimshian were primarily linguistic groups; intervillage rivalries and warfare did not give way to larger political units until the late 1860s. The intruding British presence was maritime as well, but the navy's orientation was toward deep-water vessels capable of long-distance voyages. When there was no serious threat to nineteenth-century Britain's global empire, however, the fleet relied less on ships of the line than on smaller, more mobile, and more economical steam-driven warships to protect interests in distant seas. Dispatching a gunboat became almost a conditioned response of the London government whenever the state of affairs seemed to warrant it.

The British, convinced that circumstances in the inland waters of British Columbia required it, used gunboats as the long arm of imperial and colonial governments. These boats served to survey the seas, protect trade, extend the rule of law, help missions and settlements survive, and secure the colonies of Vancouver Island and British Columbia. Sometimes their mere presence was sufficient to resolve a crisis. If trials of offenders, punishment, and other pressures failed, force was employed as a last resort. This amphibious police action, carried on in response to requests from civil authorities, cast somewhat reluctant sailor-diplomats in the role of guardians of the peace and of pathfinders of empire. Most Indians accommodated themselves well to the new order of things. Only a few resisted and were subjected to gunboat actions.

Gough has a thorough firsthand knowledge of every island, creek mouth, and cranny involved in his gunboat frontier. This is at once a virtue and a distraction. His maps are inadequate for the reader when his text plunges into the details of his familiarity. He does not, however, lose perspective but brings the reader back with meaningful conclusions. In many respects, his study is a magnificent substantive footnote to "Retrospect," his concluding chapter.

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LATIN AMERICA

JEROME A. OFFNER. *Law and Politics in Aztec Mexico*. (Cambridge Latin American Studies, number 44.) New York: Cambridge University Press. 1983. Pp. xvii, 340. \$39.50.

Jerome A. Offner maintains that in our times the Aztecs, perhaps more than other ancient societies, have become "the greatest victims of the ideological and cultural fashions and predilections that dominate certain ethnological and archaeological circles" (p. vix). It is ironical, indeed, that the Aztecs, to whom, among other things, "nutritional cannibalism" has been attributed as the ultimate reason for their sacrifices of human victims, have themselves become the victims of "researchers" who adhere to various schemes, mainly of Marxoid flavor.

It is therefore refreshing to see the appearance of a book whose author proclaims, "I adhere to no such grand scheme" (p. xv). Instead, Offner's main concern has been to center on one key aspect up to now barely investigated: the sociocultural significance of the legal systems in the kingdom of Texcoco, one of the political units in the celebrated "Aztec Triple

Alliance." That the best available sources on the legal systems of ancient Mexico are found in the Texcocan documentation of pre-Columbian tradition increases the interest of this work. In addition, what appears specifically in Texcocan law vis-à-vis the information that can be gathered about the other Nahua (Aztec) kingdoms can open new perspectives for comparison in this field of research.

The book includes a preface, six chapters, and a conclusion. After the first chapter, "The Setting and Early History of Texcocan Imperial Development," Offner enters into his real subject: Texcocan legal concerns, rules, policies, and reforms. Among other things he investigates what the legal systems contributed to the structure of the "empire" in the higher and lower levels within its political and social organization. For those who specialize in what can be described as Mesoamerican social ethnohistory, chapters 2 to 5 are rich in new insights, always supported by firsthand documentary evidence. For students of the anthropology of law and for those interested in comparative jurisprudence, chapter 6, "The Developments and Maturation of the Texcocan Legal System: Principles of Texcocan Jurisprudence," will be particularly rewarding.

As the author demonstrates, Texcocan law, mainly after the reforms introduced by the sage ruler Nezahualcōyotl, controlled many types of behavior and established how disputes should be settled in the courts. This carefully structured legal system determined the main aspects of social interrelation: organization of the *calpulli* (wards or corporate land-holding entities), class differences, kinship relations, land tenure, residence patterns, business, markets and trade, procedural dispositions, and many other aspects of the sociopolitical process and structure.

Not a few of the accepted clichés concerning the social structures of pre-Columbian Mexico are critically challenged in this book, whose contents so closely correspond to what is announced in the title. Careful use of the available sources has led Offner to produce a book that, no doubt, will be read and discussed by a wide gamut of scholars as well as other people interested in the ancient cultures of the New World.

MIGUEL LEÓN-PORTILLA

Universidad Nacional Autónoma de México

NANCY M. FARRISS. *Maya Society under Colonial Rule: The Collective Enterprise of Survival*. Princeton: Princeton University Press. 1984. Pp. xii, 585. Cloth \$60.00, paper \$19.50.

Many scholars have intensively studied the modern indigenous peoples of Mesoamerica, and others have focused their efforts on the reconstruction of

pre-Hispanic histories. But the period that separates the Spanish conquest from modern ethnographic studies has too often been neglected. Like Charles Gibson's *The Aztecs under Spanish Rule*, Nancy M. Farriss's ethnohistorical study of the colonial Yucatan Maya seeks to fill this gap.

Based on a thorough search of documentary sources in Spain, Mexico, and Yucatan and on her own astute field observations, Farriss shows that the Maya case is both similar to and different from the Aztec one. It is similar in that, before the end of the colonial period, Maya society was transformed from one with a complex social hierarchy into a simple peasant society. It is different, however, in that the Spanish conquest was less destructive in Yucatan, the native nobility lasted longer there, the "closed" and "corporate" nature of the peasant community took longer to gel, and the hacienda complex developed without the internal markets characteristic of central Mexico. This protracted sequence did not, however, mean that these differences were simply later manifestations of the same institutions, for the difference in pace also altered the nature and trajectory of the structures and processes.

This story is told in four parts. The first presents an overview of the implications of the Spanish conquest for Yucatan. Part 2 treats the physical environment, family organization, and population movements within colonial society. Part 3 constitutes the core of the work and details the basic economic, social, political, and religious changes that took place among the colonial Maya. In this section, a chapter on elites and two chapters on religion break much new ground both substantively and conceptually.

Part 4 adds important data on the often-neglected Bourbon reforms—called "the second conquest" by the author—and shows that these reforms had a profound effect on Indian society, perhaps one more pronounced than that of the initial Spanish conquest. Finally, the epilogue summarizes and comments on the processes of adaptation and survival with reference to the Maya as well as to other indigenous populations under colonial institutions.

This book is not just a "history" of the Yucatan Maya. The author relates the Yucatan situation, on page after page, to a broader context by skillfully interweaving concepts and data. Since Farriss discusses topics ranging from the origin of the state to the nature of universal religions and their effects on economic systems, some are bound to disagree with her analysis. I question, for example, her emphasis on colonial Maya elites as the main carriers of "Maya culture," for she never really explains what she means by that concept, nor does she relate "Maya" ethnicity to the vast literature on that topic.

Nevertheless, this book deserves to become a classic because it is not only the best book on colonial Yucatan but also one of the best ever on Mesoamer-

ica. It is, therefore, to be recommended to historians, anthropologists, sociologists, economists, political scientists, and anyone seriously interested in ancient, colonial, or modern Latin America. Its strongest point is that it is sheer scholarship, well conceived, researched, and written by one whose intellectual vision transcends the narrowing and arbitrary boundaries that separate the various academic disciplines in the social sciences and humanities.

JOSEPH W. WHITECOTTON
University of Oklahoma

MICHAEL C. MEYER. *Water in the Hispanic Southwest: A Social and Legal History, 1550–1850*. Tucson: University of Arizona Press. 1984. Pp. xiii, 189. \$26.00.

This work, the first on the subject, is but another of Michael C. Meyer's important publications. This one is indispensable for the study of southwestern culture and exemplary for the study of regions where the water supply does not meet human needs.

Four chapters explain the social aspects of water: culture and tradition; settlement; conflict; and social, economic, and military influences. Four chapters detail its legal aspects: law, land in relation to water, rights, and adjudication of disputes. Practices and arrangements varied among tribes, missions, municipalities, the military, individuals, the government, industries, and ranchos and haciendas, both above and below today's international boundary. Meyer explains water use, competition and compromises, and official action in Spain, New Spain, and the Southwest to regulate and to settle conflicts while serving all needs and interests. Hispanic people thought of water as "an instrument of control, a source of power, and most importantly as the fount of accumulated wealth" (p. 167). And "later Hispanic settlers remained impervious to the laws of nature," following their own "cultural predispositions" (p. 167). Water conflict did not "result from population increase . . . [but] was a product of demographic and economic change" (p. 49).

This down-to-earth history is marked by excellent background information, pertinent theory, and analytical skill and rests on hundreds of manuscripts from a dozen archives in Spain, Mexico, and the Southwest, with thorough use of the best literature (almost all of it recent). The work is illustrative, not exhaustive, with neither the "water history" of any one settlement nor information from every settlement. Problems of small cities are not explained, nor are there chronicles of droughts, tracings of progressive allocation of river waters, or data on diminishing stream flows. Review of more court cases would have shown a fuller spectrum of resolutions. Almost all new archival information dates from

1700 to 1840, and almost three-quarters comes from New Mexico, Texas, Chihuahua, and Sonora. But to mention what Meyer did not attempt is far from implying weakness in the work. Rather than establishing which processes or outcomes predominated, he sought and found many variations of adaptation to water problems. His topical approach and range of localized information yield results almost as representative of customs and usages as of policy and law. As more patterns appear, I suspect they will enhance Meyer's framework rather than fall outside it.

The author's literary style evokes good writing by eighteenth-century Spaniards and Mexicans—urbane, clear, very informative, and alert to the realities of life lurking behind facades of law, social custom, and government procedure. Meyer succeeds in picturing challenges, achievements, confusions, and disappointments of Hispanic people undergoing "ecolturation," the "human adaptation to and manipulation of a delicate ecosystem" (p. 4). This fine pioneering work will serve as a needed introduction for all who study the nineteenth- and twentieth-century Southwest, from Texas to Baja and Alta California.

CLIFTON B. KROEBER
Occidental College

CLIFTON B. KROEBER. *Man, Land, and Water: Mexico's Farmlands Irrigation Politics, 1885–1911*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xiii, 288.

Agrarian policy during the Díaz regime scarcely constitutes virgin ground on which to tread. An entire generation of historians, following in the footsteps of Frank Tannenbaum, has addressed the question, some historians hoping to unshroud the underpinnings of the Porfiriato itself, but most with the express purpose of explaining the outbreak of the Mexican Revolution of 1910. The unique contribution of this study, and the one that makes it a historiographical tour de force, is the addition of a third dimension to the conventional man-land truisms. The title clearly suggests a triad, and the text delivers on that tantalizing promise.

The modernization impulse attributed to the Díaz regime is most often seen in railroad construction, urbanization, communication breakthroughs, industrialization, and the revival of mining. But this work reminds us that there was also an important agricultural component that centered on increasing production through the application of large-scale irrigation. At the turn of the century, Mexican writers debated water policy in a series of lengthy reports. Roberto Gayol, Andrés Molina Enríquez, Francisco Madero, Leopoldo Palacios, Oscar Bran-

niff, and Manuel Vera all prepared studies and made recommendations. Some were carefully conceived, while others revealed a less-than-perfect understanding of both the juridical base Mexico inherited from Spain and the water traditions stemming from the hydrolatry of Tlaloc and Chaac. All of these studies suffered from a common problem, however. They failed to cope adequately with an obvious anomaly: Porfirio Díaz and his influential coterie of political allies could easily thwart the best plans for water reform. More important, they did not hesitate to do so when these plans ran contrary to their own vested interests.

Relying heavily on archival sources from Mexico, Great Britain, and the United States, Kroeber has crafted a work fundamental to our understanding of agrarian policy under Díaz. Having recently completed a study of water in an earlier era of Mexican history, I am struck by the continuities. Modernization generally, and the application of new technologies specifically, altered the face of Mexico during the Porfiriato, but the history of water remained largely impervious to change. The same issues that dominated the seventeenth and eighteenth centuries continued in full vigor as the nineteenth century gave way to the twentieth. How could planners minimize the negative impact of droughts and floods? How were water rights to be defined? What was the proper role of government in setting the guidelines for water use? How could the competing interests of towns, mines, industry, and agriculture be reconciled? And how could equity be attained when water was recognized and used as a weapon in a series of social and economic struggles? The answers to these basic questions during the Porfiriato differed little from those that had been posited for generations. Both before and after Mexican independence, when it came to the crucial issue of defining and implementing allocation schemes, the influence of the powerful overwhelmed the best interests of the weak.

Only indirectly does Kroeber's fine study instruct us on the outbreak of hostilities in Mexico in November of 1910. But this is not its purpose. The Díaz regime was much more than a background chapter to the Mexican Revolution. Those who read and digest the work will find it intellectually impossible to address the issue of Porfirian agrarian policy again without reference to that one resource most necessary to make man's land productive.

MICHAEL C. MEYER
University of Arizona

DOUGLAS W. RICHMOND. *Venustiano Carranza's Nationalist Struggle, 1893–1920*. Lincoln: University of Nebraska Press. 1983. Pp. xxi, 317. \$26.95.

Douglas W. Richmond has endeavored successfully to examine in careful detail the available evidence for an understanding both of Venustiano Carranza's career and his contribution to the Mexican Revolution. According to the author, Carranza has been underestimated and significantly misunderstood. Employing both archival and published materials with skill and thoroughness and using topical as well as chronological approaches, Richmond follows his subject from the final decades of the Porfiriato to his death.

Richmond's Carranza is rugged, austere, stubborn, authoritarian, and often moved by political expedience. Ideologically, the author argues, Carranza was not a conservative, liberal, or radical but rather a populist and nationalist. His concern for populist reform was tempered by his recognition of the need to reconcile competing interests. Richmond argues that Carranza was a consistently ardent nationalist who emerged, despite multiple and severe constraints, as the "1st genuine practitioner of Mexican nationalism" (p. xviii).

Carranza is a difficult person to penetrate, because his actual feelings were often hidden behind his dark glasses. Richmond wants very much to rehabilitate Carranza from the neglect and misunderstanding of the scholarly community. That difficult task at times leads the author to place his subject in the most favorable light possible. But Richmond is too good a scholar to ignore the evidence, and he does provide his reader with both sides, even if he has a tendency to glide over what reflects less favorably on Carranza.

This is evident, for example, in the descriptions of Carranza's unwillingness to take great risks by declaring for Madero, his lackluster performance early in the revolution, the meaning of Carranza's activities in the final days of Madero's government, and his negotiations with Huerta following Madero's fall. The difference between what Carranza proposed in 1917 (which hardly supports his reputed populist reformism) and the document actually enacted poses a problem for Richmond. While trying to put Carranza's role here in the best light possible, the author admits that, although the Constitution of 1917 was not what Carranza had expected, he consented to it.

Richmond contributes significantly to our understanding of the period 1913–20 when he analyzes the character of the Carrancista army and describes the first chief's key advisors and principal generals. Also positive are the sections on Carranza's reorganization of the economy and his diplomatic stance and initiative vis-à-vis the United States, Europe, Japan, and Latin America.

Perhaps the most serious problem is the author's portrayal of Carranza's opponents. Wilson and Obregón emerge as two-dimensional characters and

are subjected to unrelenting criticism. The final years of the Carranza era prove complicated for Richmond. He admits that Carranza failed to maintain his pace as a reformist and that resulted in his loss of popular and army support. Further, Carranza tolerated flagrant corruption by those closest to him. Finally, his misguided effort to impose a successor led to the final disintegration of his power. Although contending that the 1920 crisis was partly owing to the persistence of personalism in Mexican politics, Richmond concedes that poor political judgment triggered the events that led to Carranza's death. Once again we are given a negative picture of the role of Obregón, and, although the author concludes with the admission that the evidence is inconclusive, he states that "there can be little doubt that Obregón's interest was served by Carranza's death" (p. 237).

Despite these few caveats, Richmond's book shows him to be a solid archival scholar. Taken together with the Cumberland-Bailey work, this is an important contribution to our understanding of the difficult period following the Mexican Revolution. The book is also an interesting effort to rescue a major figure of the revolution and evaluate his contributions.

STANLEY R. ROSS
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FRANCISCO A. SCARANO. *Sugar and Slavery in Puerto Rico: The Plantation Economy of Ponce, 1800–1850*. Madison: University of Wisconsin Press. 1984. Pp. xxv, 242. \$21.50.

Scholars have long recognized the links between sugar and slavery in the Caribbean. Puerto Rico, however, has been singled out as an exception to the general pattern because slaves never accounted for more than 13 percent of the total population at any time. It has been assumed that the history of sugar in Puerto Rico was unique and that free labor predominated during the epoch of slavery.

Francisco A. Scarano's book should end this misconception. On the foundations of solid archival research, Scarano convincingly shows that sugar development in early nineteenth-century Puerto Rico was based on slave labor, as was the case on every other Caribbean sugar isle. He accomplished this not by examining the whole island with its densely populated regions where sugar was not produced but by focusing on one of the leading sugar plantation zones in the first half of the nineteenth century. A detailed portrait of the functional aspects of social and economic life has resulted from this microapproach. This is largely owing to the rich documentary sources available at the local level: land and slave registers, notarial protocols, popula-

tion censuses, and data on individual units of production. These kinds of sources are not available on an island-wide basis and heretofore have been untapped by historians.

This book does not simply study sugar and slave labor but also analyzes the major social and economic factors governing the growth of sugar cultivation before 1850. A chapter on the productive characteristics of haciendas in 1845 reveals that the small-scale Puerto Rican plantations (compared with their Cuban counterparts) produced high sugar yields per slave laborer and per unit of cultivated land. This may explain the lack of technological modernization on Puerto Rican sugar estates by the mid-nineteenth century as well as the absence of significant expansion of individual farms.

The central role of St. Thomas as both a source of commercial credit and slaves is well documented. The role of immigrants and incipient conflicts with Puerto Rican-born planters is discussed in another chapter that includes a case study approach. Scarano used Ponce's notarial archives to reconstruct the changing dynamics of the slave trade to Ponce and the demographic characteristics of the slave population. The mechanisms of credit and finance are explored in detail.

Scarano has been careful not to produce an "insular" history but places the development of Ponce's sugar economy within the general framework of Puerto Rican socioeconomic change, showing its connections to shifts in Caribbean production and trade patterns and to changes in world market conditions. Thus, this volume is not only a pioneering addition to Puerto Rican historiography but also a valuable contribution to the literature on Latin American socioeconomic development.

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RAYMOND CARR. *Puerto Rico: A Colonial Experiment*. (A Twentieth Century Fund Study.) New York: New York University Press. 1984. Pp. xxii, 477. \$25.00

It was an irony recognized at the time more by Puerto Ricans than North Americans, on whom, in any case, the significance would have been lost: on July 17, 1898, the new autonomist assembly of Puerto Rico convened; on July 25, the United States invaded the island and dissolved the assembly. The war to end Spanish colonialism in the Caribbean also ended self-government in Puerto Rico. Colonial administration again replaced politics as the metropolitan capital of Puerto Rico shifted from Madrid to Washington.

The history of this relationship, which Raymond Carr describes in the subtitle as "a colonial experi-

ment," serves as the subject of this study. It is an account expertly told, comprehensive in sweep, and ambitious in scope. And, indeed, it is all here: the history of the political discourse in Puerto Rico and policy discussion in the United States; a detailed account of the central question of status—commonwealth versus statehood versus independence—as seen from both sides; and, throughout, the politics, the personalities, the plottings. Carr is at his best when he sets in comparative relief the tenor of the status debate in Puerto Rico and the United States: ardor on the island, apathy on the mainland.

This work is more than a scholarly undertaking, however. It is a "project," one commissioned by the Twentieth Century Fund and given unabashedly to questions of policy. The book is, consequently, political in purpose, intended to serve as a source of counsel for appropriate administrators and authorities concerned with matters Puerto Rican. Not finding a North American or Puerto Rican scholar "who had not already made up his mind about what the relationship ought to be," the Twentieth Century Fund turned to Carr in England.

It seems that Carr, too, has made up his mind. His intolerance for *independentismo* is exceeded only by his impatience with its advocates. His assessment of *independentistas* as "a minority of malcontents" (p. 409) is not new. It may not even be incorrect. But it is a view eminently colonial in its construct, inviting obvious policy corollaries. Have not apologists of colonialism everywhere, at all times, dismissed the seers of independence as minorities of malcontents? This was certainly one view from London in February 1775 when the earl of Sandwich characterized the malcontents in the North American colonies as a "very insignificant" number of "impudent rascals." That *independentistas* in Puerto Rico are characterized as "malcontents" has political implications; that they are proclaimed a "minority" has policy connotations.

These are issues for further discussion and the subject of legitimate debate. But the quality of the discourse is not well served by the tenor of Carr's prose. He writes in mocking tones, sometimes as a function of style, often as a function of substance. There are powerful intimations of condescension in this book, sometimes strong enough to suggest disparagement of the culture and the community that are the subjects of this inquiry. Although there may be a certain Latin American style in Puerto Rican politics, writes Carr, "this does not necessarily mean . . . that Puerto Ricans are unfit for self-government" (p. 242). Does this mean that a Latin American style of politics is sometimes evidence of incapacity for self-government? And what would be the substitute?

These are unfortunate undercurrents. Much of what is substantial in this work—and much is—often

is lost in these forays of rhetoric. This remains, nevertheless, a book to be read by all concerned with the Puerto Rican "question." It will undoubtedly stand as one of the definitive books of the 1980s. Because, too, it will receive favorable attention in policy circles, the work will have some influence on the course of North American colonial policy. It is both a portrait of things past and a portent of things to come.

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BARBARA WEINSTEIN. *The Amazon Rubber Boom, 1850-1920*. Stanford: Stanford University Press. 1983. Pp. x, 356. \$29.50.

As important as the "rubber cycle" is in Brazilian history, one might wonder that solid research on that topic should be so sparse. This book by Barbara Weinstein is an outstanding contribution to scholarship on developing areas, as well as on Brazil and the rubber industry itself.

Rubber is often mentioned as a classic example of a boom-and-bust export industry, generating fabulous wealth for a few, affecting the lives of many, but not leading to the sustained growth of a diversified economy. This richly detailed, solidly documented, and well-written study identifies three broad factors to explain the specific trajectory of the rubber cycle.

One factor is the relations of production and exchange—patterns of personal dependency, exploitation, and surplus extraction—that had developed over centuries and proved quite resistant to the "rationalizing" pressures of modern capitalism when rubber demand grew and prices shot up. On the contrary, the boom brought an intensification of the complex and varied links along the chain from the tapper who gathered the latex to the landowner or local merchant who supplied and controlled the tapper to the export firm that finally sold to brokers in New York or Liverpool. In an ironic twist common in such cases, the rubber tapper's life worsened during the boom and was only somewhat eased by the collapse.

The second factor is the Amazonian ecology—a vast expanse of green overlaid with a river system providing access to the dispersed rubber trees and the sparse population that lived from sap dripping into cups. Nature has at once provided the Amazon's hope and prospect and imposed barriers only now being successfully assaulted by roadbuilders and airplanes.

The third general factor is the role of the state or, in this case, the lack of an actively developmentalist state policy. One cause and consequence was the continued political domination by a traditional elite fundamentally opposed to the changes the rubber boom entailed. This is another irony, only explainable in the context of the Amazon region's specific historical evolution. This book amply provides that context.

Weinstein's insistence on understanding what Sidney Mintz has called "local initiative and local response" in analyzing the expansion of the world capitalist system into the periphery is well taken. One cannot in any other way begin to know what people's lives were like and why and how they changed. A general discussion of causality and historical explanation would go far beyond this brief review. But the dynamic force that moves history is not necessarily in the local conditions that give the human experience its texture and makes the study of history both interesting and necessary.

Nothing in the Amazon or its past caused the demand for rubber to rise after 1850, without which there would have been no boom. Without the agents of capitalism and colonialism finding an alternative to the Brazilian source, there would have been no bust. There is little basis for the theoretical or conceptual conflict Weinstein invokes as the "relations-of-exchange versus relations-of-production" controversy, as that debate relates to the study of the peripheral areas of the capitalist world system and an understanding of history "on the ground." Both views have a heuristic value that partisans of neither side can reject or ignore.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JÁNOS M. BAK and GERHARD BENECKE, editors. *Religion and Rural Revolt*. (Fourth Interdisciplinary Workshop on Peasant Studies, 1982.) Dover, N.H.: Manchester University Press. 1984. Pp. x, 489. \$45.00.

HERBERT FREY, Religion as an Ideology of Domination. LIONEL ROTHKRUG, Icon and Ideology in Religion and Rebellion, 1300–1600: *Bauernfreiheit* and *Religion Royale*. HARVEY MITCHELL, Preface [Western and Southern Europe]. PETER BURKE, Mediterranean Europe, 1500–1800: Notes and Comparisons. CHRISTOPHER HILL, From Lollards to Levellers. DAVID NICHOLLS, Religion and Peasant Movements in Normandy during the French Religious Wars. T. J. A. LEGOFF and DONALD M. G. SUTHERLAND, Religion and Rural Revolt in the French Revolution: An Overview. CHRISTOPHER R. FRIEDRICH, Preface [Central and Eastern Europe, 1525–1848]. KARL-HEINZ LUDWIG, Miners, Pastors, and the Peasant War in Upper Austria, 1524–26. PHILIP BROADHEAD, Rural Revolt and Urban Betrayal in Reformation Switzerland: The Peasants of St. Gallen and Zwinglian Zurich. GÜNTER VÖGLER, Religion, Confession, and Peasant Resistance in the German Territories in the Sixteenth to Eighteenth Centuries. HERMANN REBEL, Bureaucratic Tenure, Peasant Leadership, and *Politiques* during the Austrian Counter-Reformation. VIKTOR I. BUGANOV, Religious Ideologies in Russian Popular Movements in the Seventeenth and Eighteenth Centuries. ALEKSANDR I. KLIVANOV, "Irreligiosity" in Seventeenth- and Eighteenth-Century Peasant Uprisings in Russia. RAINER WIRTZ, Religious Patterns of Interpretation and Mobilisation in *Vormärz* Germany. HANS G. KIPPENBERG, Preface [Middle East and Africa]. HANS G. KIPPENBERG, Limits of Islamic Civilisation: Mahdist Movements in Abbasid Iran. KURT GREUSSING, The Babi Movement in Iran, 1844–52: From Merchant Protest to Peasant Revolution. JAN-HEEREN GREVEMEYER, The Revolt of Eshaq Khan in Afghan-Turkestan in 1888: Peasant Mobilisation and Re-formation of Patron-Client Relationships. MARTIN VAN BRUINSEN, Popular Islam, Kurdish Nationalism, and Rural Revolt: The Rebellion of Shaikh Said in Turkey,

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GERHARD HIRSCHFELD, editor. *Exile in Great Britain: Refugees from Hitler's Germany*. Atlantic Highlands, N.J.: Humanities or Berg, Lemington Spa, England, for the German Historical Institute, London. 1984. Pp. 314. \$37.00.

ANTOINE E. MURPHY, editor. *Economists and the Irish Economy: From the Eighteenth Century to the Present Day*. Dublin: Irish Academic or Hermathena, Dublin; distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. 174. \$25.00

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GEORGE W. STOCKING, JR., editor. *Functionalism Historicized: Essays on British Social Anthropology*. (History of Anthropology, number 2.) Madison: University of Wisconsin Press. 1984. Pp. vi, 244. \$19.95.

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REVIEWS OF BOOKS

TO THE EDITOR:

Professor Thomas H. Etzold's review of Professor Peter Herde's *Pearl Harbor, 7. Dezember 1941: Der Ausbruch des Krieges* (AHR, 89 [1984]: 540–41), shows that author and reviewer have overlooked a basic series of Magic communications intercepts of Japanese code messages. Etzold writes: "Herde makes two special points with regard to Magic. . . . He also points out that the American lack of an espionage system within Japan limited the usefulness of what was obtained by code-breaking. The United States did not know, for example, as did the Germans and perhaps the Russians, that a Japanese decision in principle to attack Southeast Asia rather than the Soviet Union had come already in July 1941."

On the contrary, the July intercepts by the United States were the direct cause of a series of American military steps of the utmost gravity. The U.S. Army history said in 1953: "About 4 July 1941 British and American intelligence agencies became aware that the Japanese were on the verge of a major move. The United States had broken the Japanese diplomatic code, and so the President and Cabinet in early July had the full revelation of how Japan would react to the situation created by the German attack on Russia on 22 June 1941. . . . Japan would not attack Russia. . . . As a first step Japan would occupy southern French Indochina and Thailand, even at the risk of war with Great Britain and the United States. . . . The Japanese steps were soberly and earnestly debated by the President, his Cabinet,

and at the highest service levels during July's summer heat. . . . On 23 July the President approved a Joint Board paper [355]." (Charles F. Romanus and Riley Sunderland, *Stilwell's Mission to China* [Washington, D.C., 1953], pp. 22–24.)

JB 355's program was formidable: bombing Japan in November 1941; a U.S. manned and supported Chinese Air Force; support for 30 Chinese divisions; a U.S. military mission to China; federal control of U.S. exports to Japan; reinforcement of the Philippines; mobilization of the Philippine Army. The program was a landmark, for Roosevelt extended it to include an oil embargo that the armed services opposed. JB 355's recommendations foreshadowed the American effort in China during 1942–1945; it also recommended clandestine warfare. The July 1941 comintercepts caused it.

RILEY SUNDERLAND
Bar Harbor, Maine

TO THE EDITOR:

We at Ide House Publishers, after reading the review of Esther S. Lee Yao's *Chinese Women: Past and Present* by Chia-Lin Pao Tao (AHR, 89 [1984]: 826), seriously question whether or not the reviewer ever read the book—or merely skimmed the contents.

Professor Yao (University of Houston, Clear Lake) was born on Mainland China and educated on Taiwan, taking her doctorate at Purdue University.

Professor Chia-Lin Pao Tao comments that the treatment of minority women on the Mainland and on the island of Formosa is "nowhere to be found in the book." Did he fail to read pages 94f, 142–49, 189–92, etc.? The learned scholar also complains about the illustrations being inappropriate: "[only] Oracle bones, Shang bronzes, Sung Buddhist painting, Sung porcelain, and Japanese paintings of Chinese philosophers [which] bear little relevance to the subject." Yet p. 104 is a photograph of a Yunnan woman (a minority) harvesting grain, p. 177 is a photograph of Tong Ling, winner of the women's

singles at the 36th World Table Tennis Championships, p. 188 a photograph of Chinese children in contemporary competition—while the ones the review cites are in the minority of the illustrations—at the front of the book and *do* illustrate what is said.

Our dictionary (Websters' *Twentieth Century*, Unabridged) does not have "throughout" listed with a hyphen (p. 1903), although we do acknowledge and accept responsibilities for existing typographical errors not caught prior to printing, still they are not as numerous as the reviewer implies. As for transliterations, we defer to our author and our review committee which was drawn from several national universities.

Ide House, however, is grateful for the review and interest it stimulated. Out of the 105 books on woman's history we have published, it is the first review we have received. We regret, due to the economic policies at present in America, we are discontinuing publication of our projected 411 volume series (*Woman in History*), and are closing the corporation.

MELINDA RAPES
Ide House, Inc.

PROFESSOR TAO REPLIES:

It is distressing for the Ide House editors that after publishing 105 books on woman's history only one book has received a review, and that this review is not to their liking. This reviewer sympathizes with them but the above letter further exposes the quality of the book.

Due to the space limitation there is no way to detail the long list of errors here. To name a few in chapter two as examples: On page 14, "Chu Shen" should be "Hsü Shen." "Lo Mee" should read "Lo Mi." "Lu Sse Yu Pian" should read "Lu Shih Yü Lun." "Tzu Hsiu Wu Pien of Huay Nan" is a mix-up of "Hsiu Wu P'ien of *Huai Nan Tzu*." A scholar with rudimentary knowledge of Chinese classics should be able to tell that *Huai Nan Tzu* is the book title. The reference of it could even be found in John K. Fairbank's *East Asia: The Great Tradition*. All the above errors are on one page. There is a constant confusion between "matrilineal" and "matriarchal." When the original Chinese text which the author uses mentions "matrilineal" it appears as "matriarchal" (pp. 13, 15, 16, 17, 28, 33, 37, and 39). "Feng" in *Shih Ching* is translated as "wind" instead of "folk-songs" (p. 19). "Na Chia" should be "Na Ts'ai" (p. 20). "Marital relations" is in fact "marital relations" (p. 24). "De Kuei" should be "Ta Kuei" (p. 28). "Nu Kuo" should be "Nü Kua" (p. 34). "Mou Hsun" is the mistaken name for "Mou Jun-hsun" (p. 39). It shows that the author did not quote from the original article as she claimed.

The editor indicates that "throughout" is the correct spelling, which is exactly what this reviewer has been telling her. On page 105 of Yao's book it appears as "thoughtout." It is surprising that a Ph.D. in Chinese studies would depart from the customary use of "Jurchen" or "Chin" and adopt "Kings" instead (p. 98).

On the relevance of pictures, a picture of an oracle bone recording the appearance of a new star is relevant in a book on ancient Chinese civilization, Asian civilization, or even Chinese astrology. Perhaps oracle bones bearing inscriptions about women would be relevant in this book. A Sung vase with floral design is much less relevant than that with a picture of a lady. In selecting pictures one should always bear the theme in mind.

Regardless of the defects, as this reviewer wrote before, Dr. Yao's book is a useful effort in synthesizing Chinese and Western research on Chinese women.

CHIA-LIN PAO TAO
Tucson, Arizona

TO THE EDITOR:

Peter Loewenberg's review (*AHR*, 89 [1984]: 1301-02), characterizes my book *History of Bourgeois Perception* as "historicism," a form of history of ideas. Nowhere in the review did he discuss the method and the content of the book, except to mention my "project of synthesizing Marxism and phenomenology." He did not mention the influences of Maurice Merleau-Ponty and Michel Foucault on my work, but instead compared it to the Frankfurt School culture critique.

In fact, *History of Bourgeois Perception* proposes a phenomenological concept of "perception" constituted by the combination of changing hierarchy of human sensorium (cf., the works of the *Annales* school), dominant media of communications (cf., the works of Walter Ong and Eric Havelock), and epistemic order of knowledge (cf., Foucault's *The Order of Things*). Because sensorial hierarchy, communications media and epistemic order change across time, the perceptual framework constituted by them can be studied as a historical formation. This perceptual framework, itself determined by the Marxist structure of society, provides the hermeneutic context for interpreting conscious motives.

Bourgeois perception in the nineteenth century was structured by the primacy of sight in the human sensorium, the extension of typographic communications media by photography, and the ordering of things by the episteme of "development-in-time." We do not know time, space and bodily self intuitively or as "history of ideas," but rather experience

them within a perceptual context. Therefore in three separate chapters the book proceeds to "describe," within this nineteenth-century perceptual context, the new *temporality* as exemplified in experiences of time, distances from the past, visions of the future, and temporal process and immanent dynamics; the new *spatiality* as revealed in the space of production and exchange, political space, planned urban space, the private space of the bourgeois family, typographic space, and visual space; and the new *embodiment* of the perceived body, emotion, sexuality and the unconscious in the human person. Lastly, in two chapters, I show how, after the perceptual revolution of 1905–15, contemporary perception, operating under new dynamic of image communication, broke the bounds of nineteenth-century temporality, spatiality and embodiment. There is not a hint of any of the above in Loewenberg's review.

Prior to publication, my book was evaluated by Hayden White as "the most original reformulation and reconceptualization of intellectual history that I know of." More recently, David Gross in *Theory and Society* (13 [1984]: 604–09) said: "Lowe at times makes advances beyond the work of Lefebvre, Barthes, and others. But several serious problems emerge at the same time. . . . On the whole, this is a flawed but thought-provoking book."

Loewenberg's review has a hidden agenda. A phenomenological-Marxist approach to the history of perception is the exact opposite of psychohistory. The former locates ongoing, conscious motives (cf., Alfred Schutz) within the perceived social world, whereas the latter explains "motives" from the standpoint of the unconscious. From these two opposing methodological standpoints, the same set of historical phenomena are interpreted in two entirely different ways. Loewenberg could have discussed and criticized the method and content of my work with interesting results. Instead he faulted it for not being an adequate monograph in the history of ideas, and caviled at a few derivative footnotes.

DONALD M. LOWE
San Francisco State University

PROFESSOR LOEWENBERG REPLIES:

I regret that Donald Lowe is disappointed with my review of his book. He does not deny that his work is "historicist." He explains each epoch as unique in its own terms and only to be comprehended in the historical context (perception) of a given time. This is a respectable position and is not necessarily a criticism.

Lowe does not adequately define the scope of psychohistory. Besides a sensibility to unconscious processes it is the study of how men and women

have adapted to various historical exigencies, crises and stresses in a changing world. Thus what he terms the "phenomenological-Marxist approach" could be quite syntonetic with psychodynamic variables in history. There is not and need be no "hidden agenda" of "opposing methodological standpoints" which Lowe believes he detects.

Those interested should read Lowe's book and judge for themselves whether it is useful and whether he in fact does what he claims.

PETER LOEWENBERG
University of California,
Los Angeles

TO THE EDITOR:

In his review (*AHR*, 89 [1984]: 1304), Martin Berger concedes having difficulty in assessing my "curious work," *Marx as Politician*. I suggest that his problem arises from the simplicity of the book's thesis, which runs counter to the current of Marxian scholarship. The study of Marx and Marxism has become a theological exercise intoning such hallowed formulations as dialectics, base and superstructure, materialism plain and historical, and consciousness true and false.

The book is easy enough to categorize—something Berger fails to do—as a political biography, and its thesis, developed in a straightforward narrative interrupted for requisite analysis, is that Marx was a master politician of revolution. It points to a highly visible Marxism institutionalized today in the form of governments ruling more than a third of the world's population and converted into mental patterns in the heads of virtually everybody capable of thinking in political terms. The book can show Marx consciously, as expressed in his own words (in letters to Engels most specifically), seeking to achieve such effects.

Berger's reading has been diligent but it missed the connections I drew between the living Marx and his posthumous success. This includes Marx's instinctively sure leadership of various more or less revolutionary organizations from Communist League to First International, his nurture of the German Social Democracy, and his linking up with Russia's revolutionary populism. It also includes his adept public relations techniques in creating martyrs and myths, as in the case of the Cologne Communists on trial in 1852 and the Paris *Communards* of 1871. Masterfully manipulating and propagandizing, Marx was a great political leader, even if most of his contemporaries overlooked him and contemporary Marxists prefer not to notice.

Similarly, preferring to see Marx as a helpless refugee in London, Berger writes off his work on *Capital* as a private event in the history of his psy-

che—"an escape from politics, a by-product of Marx's inability to control contemporary political events." This is to apply a remarkable reductiveness to a colossus of Western culture.

While vainly seeking an appropriately convoluted Marxian thesis, Berger nevertheless demands a simple Marx—exclusively saint or sinner. But human character is always mixed. Marx had genius, integrity, and the staunchness to make and keep a lifetime commitment to a great cause. He was also a mass of nasty qualities, a *monstre sacré* much like those equally unbelievable personalities of his time, Victor Hugo and Richard Wagner.

DAVID FELIX
Bronx Community College and
Graduate Center,
City University of New York

PROFESSOR BERGER REPLIES:

I welcome David Felix's reiteration of his book's thesis, and I agree that dealing with applied, revolutionary Marxism is a welcome alternative to concentrating on theoretical, metaphysical Marxism.

But the thesis of *Marx as Politician* is by no means simple. It is no simple thing to write a political

biography of someone whose political effectiveness, in his own time, was achieved through disciples who—in Marx's opinion—misunderstood, garbled, or betrayed Marx's views. A leader who is overlooked and unnoticed, but is nevertheless a great political leader, is a complex phenomenon.

The connection between the living Marx and his posthumous successors is indeed the core of Felix's interpretation, as my review noted. It is this connection which makes the book so original and so curious. Felix insists that Marx, through his writings and his example, not only controls the avowedly Marxist successors who disagree so fiercely with one another, but controls everyone else as well. "Virtually everyone capable of thinking in political terms" has Marxist patterns in his head; "we are all Marxists," Felix concludes in his book. How it is that Marx controls us all from Highgate Cemetery, and why he controls different people in such different directions, seem to me matters quite as mysterious as most of the theological Marxian exegeses that Felix deplores. Felix may have found Marx standing on his head in much contemporary scholarship, but it is by no means clear that he has set Marx right side up.

MARTIN BERGER
Youngstown State University

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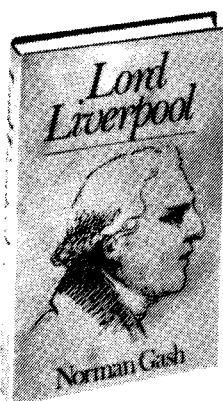
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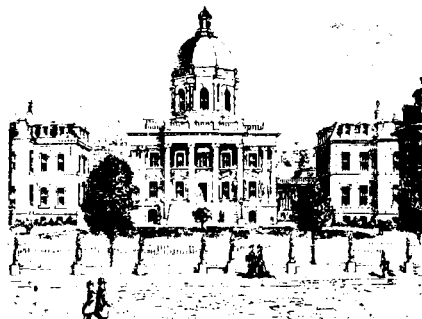
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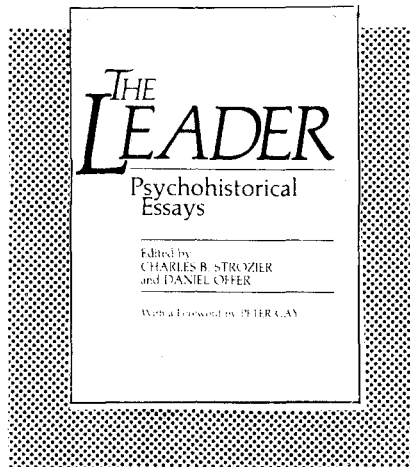
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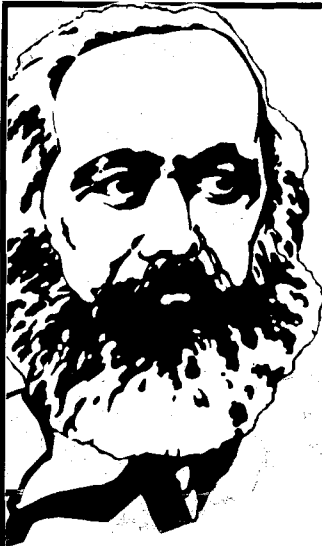
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